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THE QUARTERLY JOURNAL OF EXPERIMENTAL PSYCHOLOGY

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THE QUARTERLY JOURNAL OF
EXPERIMENTAL PSYCHOLOGY

Vol. III 1951 Part 1

AN EXPERIMENTAL CONTRIBUTION TO THE PROBLEM OF APPARENT REALITY

BY

MARGARET R. PHEMISTER

(From the Laboratory of Experimental Psychology, Louvain)

Certain two-dimensional figures acquire a character of relief and reality when oriented obliquely to the observer and viewed monocularly. The experiments were made with two types of figure—a simple straight line, and more complex patterns of lines. The results indicate that the character of reality gradually increases as the experimental conditions become more favourable. The conditions found to be most favourable are dissimilarities of size, brightness, microstructure, shape, perspective clues, between figure and ground.

When the segregating factors are operating, the figure becomes independent of the paper and takes up its own orientation in space. In the case of the simple line, this orientation may be frontal, but more usually the orientation of the background is taken into account, the resulting structural organization being that of a "needle" projecting from the background. The reality is greatest when the orientation of the background is such that the "needle" appears to be perpendicular to the ground. The complex figures used in the experiments were perspective designs of parallelepipeds. Again, the oblique position of the design associated with the greatest degree of reality of the tri-dimensional object is that which permits the figure to be seen as projecting perpendicularly from its background.

Il y a certaines figures à deux dimensions qui se détachent d'un caractère de réalité quand on les observe en vision monoculaire et en position oblique. Pour les expériences on utilise deux espèces de dessin—une simple ligne droite, et des figures plus complexes. Les résultats montrent que le degré de réalité s'augmente en tant que les conditions expérimentales deviennent de plus en plus favorables. On trouve que les conditions les plus favorables sont des dissemblances de grandeur, de clarté, de microstructure, de forme, de perspective, entre la figure et le fond.

Les facteurs favorables donnés, la figure se détache du papier et se tient dans l'espace avec sa propre orientation. Pour la ligne droite, cette propre orientation peut être fronto-parallèle, mais plus souvent on tient compte de l'orientation du fond, et il en résulte que le champ s'organise comme une "aiguille" plantée sur le papier. Le maximum degré de réalité se produit quand l'orientation du fond est telle qu'elle parait être perpendiculaire à l'aiguille. On faisait aussi des expériences en employant des dessins de parallélépipèdes en perspective. On trouve encore le maximum degré de réalité quand l'orientation du dessin est telle qu'on le voit comme object tri-dimensionnel qui adhère perpendiculairement au papier.

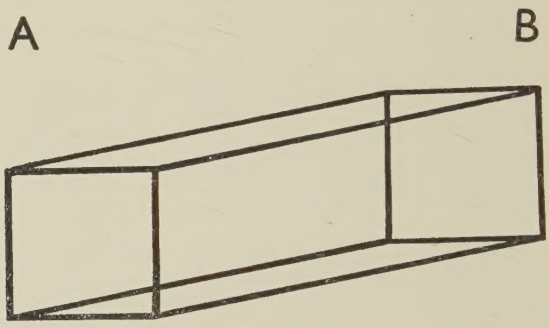
I

Introduction

The present work is the report of an experimental study concerning the problem of apparent reality in perception, which was raised recently by Michotte (1948) in an article on perspective in linear design. In this article, Michotte drew the distinction

between reality and tridimensionality; tridimensionality is possessed by both solid objects and perspective drawings and is defined by the possibility of its equation with the volume of a solid object. The character of reality, on the other hand, is related to the possibility of manipulation, or rather, to the belief in the possibility of manipulation, of the form and is possessed by solid bodies but not by perspective drawings. However, it is possible for a design to acquire this character of reality under certain conditions, and Michotte gives a particular instance of this: an elongated perspective figure of a parallelepiped, if looked at monocularly and from a very oblique angle, becomes free from its background and looks like a real object, attached to the paper by one face only (*vide* Figure 1.)

FIGURE 1 (MICHOTTE *op. cit.*)



The figure should be looked at obliquely and monocularly, the eye being in a position almost in the same plane as the paper and preferably on the side B of the figure.

A simpler form of the same illusion was reported many years ago by Ladd Franklin (1887; 1896; quoted by Sanford, 1898): a number of short lines are drawn on a sheet of paper so that they all converge towards one point. Those lines are then looked at monocularly, the eye being placed in the position of the point of convergence of the lines and the sheet of paper being held almost horizontally on a level with the eyes; the lines them seem to detach from the paper and look instead like a group of short vertical sticks. The conditions of this phenomenon, as stated by Ladd Franklin, are as follows:

> "Lines which have any position whatsoever in planes passing through the axis of the body (or, for small near objects, in planes passing through the vertical meridian of the eye with which they are looked at) are taken by us to be vertical lines."

The converging lines throw their images on meridians of the eye very close to the vertical meridian and thus tend to be seen as vertical lines. The effect is reinforced by our experience that a large number of parallel lines are generally verticals, e.g. sides of buildings, trees, etc.

The same illusion arises when a single line is used; the line becomes free and appears to be *really* perpendicular to the paper. That is, it appears to have an independent existence and to be no longer merely a "trace" on the paper. The distinction between the "trace" character of the line and its appearance as something standing by itself and extending in space beyond the plane of the paper may be

shown in a very simple way. If a simple V is drawn on a sheet of paper it can be seen as the perspective representation of a vertical stick standing perpendicularly on a plane which has as one of its features, a line which meets the base of the vertical stick. But if the V is looked at monocularly from an oblique angle, one arm of the V seems *really* to be detached from the paper and looks like a vertical stick, while the other arm remains in the plane of the paper.

It seemed that a systematic study of the illusions quoted by Ladd Franklin and Michotte would yield data relevant to the problem of apparent reality. This, then, is the purpose of the experiments reported in the following pages.

Ladd Franklin claimed that the condition which was necessary for the occurrence of the illusion was that of stimulation of the vertical meridian of the eye. The adequacy of this explanation seemed doubtful from the start, and the experiments were therefore devised with a view to discovering whether or not this was the only significant factor, and, if not, what were the other factors at work in this phenomenon. Two fundamental cases were studied:—

A that of a simple straight line;
B that of a more complex pattern.

II

Simple Straight Line

Technique A.

The aim of the experimental procedure was to allow an observer to look monocularly at a given figure which could be presented in a variety of positions, more or less oblique to the frontal plane of the observer. The apparatus consisted of an optical bench, the arm of which was set parallel to the line of regard; card-frames which were rotatable about their vertical axes could be fixed at any given distance from the observer. The head of the observer was held steady in a head-and-chin rest, the height of which could be varied; by this means it was ensured that the eyes of each observer were on the same level, so that the horizontal meridian of the eye and the horizontal axis of the card-frame should be in the same horizontal plane. The general background of the apparatus was non-homogeneous—glass and woodwork of apparatus cases: in this way, the observer was better able to see the true orientation of the card frame than if the general background had been a homogeneous screen. During the experiments, the subject's left eye was covered; both eyes were closed between successive presentations. The presentations were made in a series which commenced with the frame set in a frontal position; the frame was then rotated clock-wise, so that the angle between the frame and the line of regard gradually decreased; the actual angles of presentation being 90° (frontal), 70°, 50°, 40°, 30°, 25°, 20°, 15°, 10°, 5°. For each position of the frame, the subject was asked to report on the appearance and location of the line.

Three subjects took part in this group of experiments. Each experiment was made only once with each observer, except in the case of the first observations that were made. These were carried out with lines of different lengths, these series being presented three times to each subject. Study of these results suggested that a fairly clear general picture could be obtained by presenting only one series to each subject.

Discussion of the Group A Experiments.

The first experiments were made with a horizontal black line of length 6·6 cm. and width 1 mm. which was placed centrally on a square white card of side 20 cm. From these first experiments certain general conclusions could be drawn which were supported by all the later experiments.

Firstly, it is clear that the impressions which arise depend on the angle between the plane of the card and the line of regard. At the more acute values of this angle, the line appears independent of its background and much shorter than it appeared in the frontal position; as is well known, size constancy holds only within a limited range. Change of impression is gradual; when the card is presented in the frontal plane the line is clearly seen to be designed on the paper, but by 70° it begins to free itself from its background, and this freedom gradually increases until by 30° there is usually complete freedom, though sometimes this occurs as early as 50°.

Secondly, it is necessary to distinguish three different types of structural organization which may arise:

(*a*) The line sticks to the paper and is seen in the same perspective as the whole frame, but there is practically no constancy of length, and phenomenally the line becomes gradually shorter as the angle of presentation decreases.

(*b*) The line achieves a greater or less degree of freedom from the paper. Even in the frontal plane, although it appears to be *on* the paper, it does not strictly belong to the paper. In the case of our technique, using black lines on plain white paper, it is particularly noticeable that there is in the line no trace of the microstructure which is apparent in the surrounding paper. As the angle of presentation decreases, the distal end of the line becomes more and more free of the ground, while the freedom of the proximal end increases to a considerably less extent. Consequently, the line is often seen as curved or broken. At the most acute angles, the independence of the line increases to such an extent that the line touches the paper only at its proximal tip, and it is seen as a "needle" sticking into, or sticking through, the background. The angular relation between the "needle" and the paper is at first, usually, somewhat acute but gradually increases towards perpendicularity for the smallest angles of presentation.

(*c*) The line achieves complete freedom from the ground; it appears to be located in the frontal plane and quite unrelated to the paper. The location and appearance of the ground are not very clear; sometimes it also seems to be frontal, of trapezoid form, but still separated from the line; more often it has the appearance of a "fog" or film colour with no defined limits.

Thirdly, it seems that the experimental conditions are ambiguous, inasmuch as identical physical conditions may give rise to these differently structured impressions, either on different occasions or during a single prolonged observation, when there may be succession or alternation of two or more types of organization. It is possible, also, that different observers may favour different types of structure. For instance, Subject B perceived almost exclusively the first two types of structural organization, rarely seeing the line as if it were located in the frontal plane. Subject K, on the other hand, seemed to favour the frontal structure, perceiving this organization at an earlier stage of the series than he could perceive the "needle" organization.

Other factors influencing the type of structure perceived on any given occasion include: the structure which dominated in the immediately previous presentation; the types of structure seen in previous experiments; more specifically, an attempt to estimate the length of the line results in the favouring of the type (*c*) (frontal) organization.

As a consequence of the gradual change of impression, the diversity of possible structures, and the ambiguity of the responses, it is difficult to formulate any definite rules as to the precise conditions corresponding to the emergence of an impression of relief and reality. But already, these experiments, by utilizing a horizontal line

instead of a vertical line, have disproved Ladd Franklin's claim that verticality is a necessary condition.

In order to ascertain whether or not vertical lines have any privilege over lines which throw their images on the horizontal meridian of the eye, the apparatus was changed in such a way that the same card (with the 6·6 cm. black line) was presented so that the line was vertical and the card was rotated about the horizontal axis instead of the vertical axis. The same series of angles of presentation and the same conditions of observation were employed. The results from these series were almost identical with the results from the horizontal line. As a further control, two more cards bearing straight black lines of 15 cm. and 3 cm. were presented so that the lines were vertical and rotated about a horizontal axis. The results from these were again similar to the results obtained when the same cards were presented so that the lines were horizontal and rotated about a vertical axis.

A second change in the apparatus enabled us to present the frame in any oblique position, with the axis of rotation also oblique, instead of either vertical or horizontal. Again the figure used was the 6·6 cm. black line on the 20 cm. square white card. Initially the frame was set so that the line was horizontal; then the frame was tilted sideways about the sagittal axis so that the axis of rotation which had been vertical was now oblique. The frame was then rotated about this axis in the usual manner and as in the previous experiments, the line frees and stands out perpendicularly from the card for the smaller angles of regard. The strength of the impression of reality is in no way less than that obtained when the presentation was made about a horizontal or a vertical axis of rotation. In fact, if the acute angle between the card and the line of regard is maintained and the whole frame is rotated round the sagittal axis, the observer perceives no change in the quality of the impression, but merely sees an oblique white plane, out of which projects a short black needle, the whole thing being rotated in space. It is clear that throughout this experiment the line threw its image on some meridian of the eye and that this meridian was always perpendicular to the meridian which was parallel to the axis of rotation of the card.

These experiments provide further evidence in support of our criticism of the theory of Ladd Franklin, since it seems that exactly the same results are obtained whatever the position of the line or the plane, provided that the plane is rotated about an axis perpendicular to the line.

All the following experiments are classified into six groups, each of which throws light on a specific factor which it seemed might have some influence on the phenomenon.

1. To start this more detailed study, it was decided to investigate the possible influence of having *the line oriented on the card in such a way that it was not perpendicular to the axis of rotation*, or to impose fixation conditions which made it impossible for the subject to see the line as perpendicular to the axis of rotation. For the first part of the investigation, the material consisted of a set of cards, 20 cm. square with straight black lines of length 6·6 cm. and width 1 mm. designed on them at angles of 5°, 10°, 15°, 20°, 30°, 40°, 50°, 60°, 70° to the horizontal, in such a way that the proximal end of the line always occupied the same point of the card. The centre of the card was 40 cm. from the right eye of the subject. The procedure and apparatus used was that described in Technique A. Though no point of fixation was imposed, it is probable that the observers viewed the line in such a way that its image fell on one of the oblique meridians of the eye, and so it was impossible for the subject to see the line as projecting perpendicularly from the background which was rotated round a vertical axis.

The results from this series of experiments show that oblique lines also achieve a certain degree of freedom from their background, but that this freedom is not so pronounced as in the case of the horizontal line. The strength of the impression of freedom appears to decrease as the angle between the line and the horizontal increases and disappears altogether for small angles of presentation when the projection of the line on the retina is almost vertical, i.e. almost coincident with the axis of rotation, the line then appearing as a vertical trace on the vertical card.

Some individual differences are noticeable, making it therefore impossible to indicate clearly defined limiting positions associated with the different impressions. As previously noted, Subject B shows distinct preference for the "needle" type of structure. And, of the three subjects, M perceives most readily the true perspective structure of the field, while B most easily sees the line freed from its background as a real object. But for all three subjects the type of change in impression was similar as the conditions varied; for instance, for two presentations such that M perceives the line, in one case, as designed on the paper and, in the other case, as a needle with a small degree of reality, B is able to perceive the former as an unreal needle and clearly sees the second as a very real needle. In other words, the trend of the changes is the same, but it would be possible to give definite quantitative measures of the conditions corresponding to the different types of impression, only if large numbers of subjects were used and the results treated statistically.

The second part of this investigation was to arrange the conditions so that the stimulus line did not throw its image on a meridian of the eye, and so could not be seen perpendicular to the axis of rotation of the card. A horizontal black line, length 6·6 cm. and width 1 mm., on a square white card of size 20 cm. was presented to the subject four times in succession: the first time, the observer was asked to fixate the proximal end of the line; the second time, to fixate a point 1 cm. below this end of the line; the third time, to fixate a point 2 cm. below the end of the line; the fourth time, to fixate a point 3 cm. below the end of the line.

The responses showed that as the image of the line became farther removed from the meridian of the eye, the more difficult it became to see the line projecting from the paper. Fixation of the tip of the line favoured the freeing of the line, and gave the "needle" structure of the field preference over the "frontal" structure. For all the series where the point of fixation was below the line, the impressions were not altogether clear, even for the frontal presentation of the card. From the point of fixation nearest the line, the line was seen as a needle for the more acute angles of presentation, but not as easily in this series as when the line itself was fixated. From the point of fixation 2 cm. below the line, the line was seen as a needle only for one or two of the most oblique positions of the frame, and this organization was not very stable. The "needle" organization was not achieved at all when the lowest point was fixated. The lack of sharpness of the line in these latter cases made it difficult to define any relation between the line and the paper and so the line was often seen in a frontal plane.

Summarizing the results of these experiments, we know that a line will detach from its background and take on a stable character of reality only if its retinal image falls on, or near, a meridian of the eye. We also know that the character of reality of a line is very weak if the line is not drawn perpendicular to the axis of rotation of the ground. Since we have already shown that the illusion can arise whatever meridian of the eye is stimulated, the absence of the illusion in this case cannot be attributed to the absolute orientation in space of the line; hence, we may conclude that it is the condition of perpendicularity between line and plane which is significant. To illustrate the truth of this, the apparatus was changed so that the

frame could be rotated about all three orthogonal axes. The material used was the set of cards bearing lines which were oblique with reference to the coordinate axes of the cards. Here again it appeared that by adjusting the position of the plane with respect to three axes, it was possible to find an orientation of the plane which enabled the observer to have a strong impression of reality from the line. When this orientation of the plane was achieved, it was always found that the relationship between the "needle" and the plane was that of perpendicularity.

2. The next step was to discover what influence the *length of the line and the size of the ground* might have on the illusion. For the first observations, the size of the card was kept constant, 20 cm. square, the breadth of the black lines was kept constant, 1 mm., but the lengths of the lines were varied. Three different lengths of horizontal line were used— 3 cm., 6·6 cm., 15 cm. Following this, observations were made using smaller cards, again white, 10 cm. × 6 cm., with lines of width 1 mm., and lengths 3 cm. and 6·6 cm. The cards were presented so that the lines remained always on a level with the horizontal meridian of the eye and the method of presentation was that described on p. 5. The vertical axis of rotation of the frame was 100 cm. from the eye of the observer.

From the responses to these experiments it appears quite clearly that a short line frees more readily from its background than does a longer line. But that this is a question of relative length rather than absolute length is shown by the fact that lines of equal length on different-sized backgrounds tend to stick more readily to the smaller ground; hence the line of 6·6 cm. shows less freedom from the small card than from the large card; and this is also true of the line of 3 cm. although its degree of freedom from the card still remains greater than that of the 6·6 cm. line.

It seems, too, that at our observation distance, the smaller cards are more easily seen in their true orientation than the larger cards, which readily take on an indefinite, foggy appearance and therefore lend themselves rather to the "frontal" type of organization than to the "needle" type. Thus the absolute size of the card may affect the position taken up by the line when it is free, but it is the relative sizes of the line and the ground which determine whether or not the line shall become free in the first place.

To complete this section of the investigation, an experiment was made with a white card, 20 cm. square, which had a line drawn horizontally across it, from edge to edge, representing the limiting case of equality of size of line and ground. The results from this experiment were exactly what one would expect from such conditions —there was absolutely no tendency to freedom of the line, which remained completely integrated with the card for all positions of presentation and appeared as a dividing line belonging to the contour.

3. The possible variation of impression with *change in distance* between figure and observer was the next problem to be examined. The cards used here were the three large cards, 20 cm., bearing the lines of lengths 3 cm., 6·6 cm., and 15 cm. These cards were presented at a distance of 40 cm. from the subject, and the responses compared with the responses given when the same cards were presented at 100 cm. from the subject. The most striking difference in the results is the greater ease with which the lines are seen in perspective when the cards are presented nearer the subject. This is achieved, at least in part, as a consequence of the difference in sharpness of the ends of the lines, the difference in sharpness being shared by the two edges of the card. Probably at short distances, since the similarity of perspective clues from the line and the card is then more apparent than for greater distances, this similarity goes to unify the figure and the ground.

4. A necessary condition for the illusion is the use of *monocular vision* and so this must be listed as one of the significant factors at work in the phenomenon. In binocular vision, the presence of binocular parallax which acts in the same way for both the line and the card, results inevitably in their being seen as a unit, a line traced on the card.

5. The material for the next set of experiments was devised with a view to discovering what influence the factor of *brightness difference* would have upon the phenomenon under investigation. The material consisted of three white cards, 10 cm. × 6 cm., one having a pale orange line, 3 cm. × 3 mm., designed on it; another having a darker orange line of the same size; the third having a black line of the same size. The figures were presented at a distance of 100 cm. from the observer.

The lack of ambiguity or individual differences in these results makes it quite clear that the greater the similarity of brightness of figure and ground, the more strongly unified the two remain; and it is only when there is a very marked difference in brightness, that the line achieves total freedom and is seen as projecting from the ground, for very acute angles of presentation.

6. During the first experiments it was remarked that even when the figure was presented in the frontal plane, it had a certain amount of independence, and we thought that this might be due to the fact that there was no trace of the microstructure of the ground apparent in the figure. To determine whether or not this difference in *surface appearance* was an influential factor, it was necessary to discover whether or not the illusion still occurred when the microstructure of the ground and figure was the same. For this purpose, small transparent coloured rectangles were designed on cards, 10 cm. × 6 cm., of mottled grey paper and of white paper of very rough surface. These irregularities of surface appearance were visible through the transparent figures. Control cards of the same material but with heavy black rectangles of the same size, through which the microstructure of the ground was not visible, were also used. These figures were presented at a distance of 100 cm.

The results from these series of observations were very striking, in good lighting, when the microstructure was clearly visible, the transparent figures were seen persistently as coloured patches on the surface of the paper; while the opaque black figures achieved a fairly strong degree of freedom. These results may be partly attributable to differences in brightness; but that they are in large measure due to the differences in microstructure is shown by the fact that in weaker illumination, when the microstructure is not clearly visible, the figures showed some tendency to freedom.

III

General Conclusion to Group A Experiments

These experiments have made it clear that there are a number of factors which may be favourable or unfavourable for the freeing of a drawing from its background. Any factors which help to unify the figure and the ground, similarity factors, prevent the figure from becoming independent. Factors which help to segregate the figure and ground favour the perception of the figure as something standing on its own, with the appearance of reality.

All the factors can be listed in such a way as to show how they can operate favourably for the phenomenon:

Orientation of the card in such a way that the line may be seen as perpendicular to the background.

Marked difference in size between figure and ground so that there is no tendency for the line to form a whole with the contour of the card.

Lack of similarity of perspective clues from figure and ground—in particular, the perspective clues which arise when there is not a very great distance between figure and observer.

Use of monocular vision.

Strong brightness contrast between figure and ground.

Strong microstructure contrast between figure and ground.

If the converse factors are operating, the figure belongs entirely to the ground.

In conclusion, we may say that as long as there are integrating factors present, the line belongs to the card, or sticks to the card, and is seen as being drawn on it. On the other hand, when segregation factors are at work, the line is freed from the card. And, this being so, we might expect, *a priori*, that the line would always appear in the frontal plane, independent of the card, since the line always appears in the frontal plane when it is seen through a reduction screen. This is what actually happens in structure (*c*) (p. 4); but, generally, since there is no reduction screen, the card and its orientation in space are clearly seen and it is therefore seen as a real object tilted in space and not as a film colour. Consequently, we are having to do with a more complex structural organization including both line and card. Under these conditions, our experiments have shown that the maximum freedom of the line is given when the line seems to be *perpendicular* to the card. It seems, then, that this perpendicular structure may be considered as a pregnant structure or pattern. And this is shown by the fact that in such cases, the line does not appear to lie in the frontal plane (as it does in structure (*c*)) but instead its position is determined by the perpendicularity as such. There are, then, two possible "freedom" structures—total independence, or integrated structure of line and background which possesses most freedom when it has the favoured perpendicular form.

To show that the perpendicular organization of the field was immediate, a tachistoscopic experiment was made. The field presented in the tachistoscope was a white card, oriented obliquely to the line of regard, the card bearing simply a short black line. The point of fixation corresponded to the position of the proximal end of the line. In tachistoscopic presentation to four subjects, this field was organized immediately in the structure of an oblique white card with a short black pin projecting from it. Thus the freedom of the figure from the ground seems to be immediately given, independently of eye-movements. A control experiment, using a card with a real pin projecting from it, showed that the observers perceived no difference between the card bearing the line and the card bearing the real pin. The subjects for this experiment had all taken part in previous experiments and so these results might be influenced by training; this should be controlled.

IV

Experiments B. Complex Patterns

The use of a complex pattern immediately introduces a change in the quality of the perception: many such figures can be seen even binocularly as perspective drawings, and certain ones, such as cubes, parallelepipeds etc., have a special perspective value. So that these complex figures have a stronger potentiality of reality than a simple straight line. We may start, however, with the simplest cases. Reference has already been made to the case of the V which is seen, under conditions of monocular vision and acuteness of angle of regard, as a vertical stick standing on an oblique plane which has a line designed on it which meets the base of the stick. This "stick" has much more reality than has a single line seen under the same conditions. The strong impression produced by the V is demonstrated by slowly turning the paper bearing the V, still looking at it monocularly and obliquely, so that the arm

which first appears as a stick gradually sinks into the plane of the paper and the other gradually rises out of the paper and becomes the stick. The presence of the second arm of the V, by its dependence on the paper, seems to accentuate the independence of the first arm. A further reason for the stronger impression of reality aroused by a complex pattern is probably to be found in the fact that a complex of lines helps to give more structure to the ground as well as to the figure; and hence it helps to define the relation between the figure and the ground. And, as it has become clear from the previous group of experiments that the angular relation between the figure and the ground is of great importance, any conditions, such as complexity of design, which help to emphasize any such favourable relationship will necessarily enhance the character of reality of such a figure.

This was shown strikingly in an experiment using a white card, 20 cm. square, with a black horizontal line, 6·6 cm. long and 1 mm. wide, drawn on it and crossed at its proximal end by a short, thick, black vertical line. The card was presented to the subjects according to Technique A (p. 3) and the differences between the responses to this figure and the responses to the simple line of the same length on a card of the same size, are very marked. The horizontal line of the T-shaped figure was seen as projecting from its base, with a strong degree of reality, and never seen as a completely detached line in the frontal plane with no defined relation to the background, as often occurred with the plain line.

A further series of experiments was made with a variety of more or less complex patterns. The figures presented were of two types: simple contour figures whose outline was $\pm$ 1 mm. thick; and similar contour figures enclosing a straight horizontal line of length 6·6 cm. and width 1 mm.; these figures were designed on white cards, 20 cm. square, and were presented at a distance of 100 cm. from the subject. The technique of presentation was the same as that of the Group A experiments (p. 3). The simple contour figures included: square; rectangle with long horizontal axis; cross with long horizontal arm. The figures enclosing the straight line were: narrow rectangle; figure with straight sides and curved ends—two of these were used, one being very narrow, and the other being less narrow so that the sides of the figure were farther removed from the enclosed line.

The results from these experiments indicate that a figure whose perspective follows that of the card is seen most easily in the plane of the card. Not only is it true that such a figure is seen more easily in the plane of the card, but if it encloses a simple straight line sufficiently closely for the figure and the line to form a unit, the line also is held to the plane of the background. Exceptions to this general rule are found when the frontal form of the figure, i.e. the stimulus form, is as pregnant as, or more pregnant than, the perspective form. This question of pregnancy will be shown later to be of major significance.

Some experiments were also made with simple parallel lines, but the results were fairly inconsistent among the different observers and several very different structural organizations were produced.

The final experiments were made with true perspective figures. Two slightly different designs were used, but the method of producing them was the same. A real wire parallelepiped of square cross-section 5 cm. and length 8 cm. (thickness of wires, $\pm$ 1·5 mm.) was set perpendicularly against a white vertical background, 40 cm. square, so that the posterior square face of the model was flat against the background and the sides of the model were horizontal and vertical. This parallelepiped was then photographed, but in order to have a good perspective image, the camera was placed sideways towards the right and at a higher level than the parallelepiped. For the first photograph, the distance from the centre of the

model to the lens of the camera was 87 cm.; the "horizontal" angle (i.e. the angle between the horizontal projection of the optical axis of the camera and the plane of the background) being 71° 20′, and the "vertical" angle (i.e. the angle between the optical axis of the camera and the vertical axis of the background) being 74° 15′. A second slide was made under the conditions: distance from lens to centre of model, 81 cm.; "horizontal" angle, 69°; "vertical" angle, 81°. Several identical copies of these exact perspectives were made on photographic paper by projecting the slides on to a screen under the same conditions of distance, vertical angle and horizontal angle; so that the image thrown on the screen was the exact perspective representation of the real figure and on the same scale as the real figure.

Technique B (1).

Two of the identical designs made from the first slide were used in this first set of experiments. One was fixed in the "correct" position, at 87 cm. from the eye of the observer, at a "vertical" angle of 74° 15′, and at a "horizontal" angle of 71° 20′, so that the image thrown on the retina by this design should be exactly the same as would have been thrown by the real parallelepiped if the eye of the observer were in the position of the objective of the camera. The other design was fixed on a frame whose centre was 87 cm. from the eye of the observer and on the same level: the observer was able to change the position of the frame which could rotate about its vertical and horizontal axes. The subject, whose head was fixed in a head-rest, was required to compare the two figures and vary the position of the frame so that the difference in reality between the two figures should be maximum. After this first adjustment was made, the subject was asked to alter the position of the frame again in order to find some other orientation which gave an equally strong impression of reality. These double adjustments were made five times by four subjects: all the subjects agreed that the *impression of reality* evoked by the exact design as seen from the exact point of view was considerably less strong than the impression of reality called out by the same design seen from a different point of view, and the required adjustments were made without any difficulty. It should, however, be noted that when the design was adjusted to the position of greatest reality, the apparent shape of the parallelepiped was quite different from that of the control figure.

Results and Discussion of Experiment B (1).

In the course of the adjustments of the position of the frame, there was relatively small range of variation in the tilting about either axis. The initial frontal position of the frame is taken as being represented by angular values, about both axes, of 90°. If the results are divided into groups of roughly comparable sizes and the averages of these groups are taken, the frequencies of the adjustments for the different couples of vertical and horizontal angles are:

Vertical angle (i.e. *tilt about horizontal axis*)		*Horizontal angle* (i.e. *tilt about vertical axis*)		*Frequency*
Average	*Range of group*	*Average*	*Range of group*	
54·5°	—	50°	—	1
48°	51°–45°	58°	65°–53°	8
41°	44°–39°	52°	58°–45°	10
37°	38°–34°	46°	55°–44°	6
31°	33°–29°	45°	53°–37°	7
24°	28°–21°	44°	51°–48°	6

(two subjects made only a single adjustment at the first presentation bringing the total number of adjustments to 38 instead of 40).

It is to be noted that these angles are not *immediately* comparable with the positional angles of the control figure in this experiment, because of the difference of position of the two drawings relative to the eye, but the results do indicate that certain combinations of vertical and horizontal angles evoked impressions of reality much more frequently than did other combinations; and, more particularly, that the greater the horizontal angle, the greater must also be the vertical angle to produce a strong impression of reality; and vice versa.

Taking the averages of the first, preferred, adjustments made by the subjects during the five series of observations, we have—

Subject P ..	Av. vertical angle	45°	Av. horizontal angle	50°
Subject M ..	,,	43°	,,	55°
Subject K ..	,,	39°	,,	50°
Subject B ..	,,	30°	,,	42°

The figures for the first three subjects suggest that there seems to be an optimum position for the appearance of reality of the design, and that this position is certainly very different from that corresponding to the real perspective. The results from Subject B were not altogether consistent with the results of the other three subjects, but it was discovered that the form of figure appearing most real to this subject was different from that preferred by the other subjects. This will be discussed later. At this stage of the experiments it was sufficient to note three general points.

1. A given design of a parallelepiped evokes a less strong impression of reality when it is set in its correct position, corresponding to the exact position of the real parallelepiped from which the design was made, than when it is set in some other position where there is a more acute angle between the plane of the design and the line of the regard. In other words, a drawing does not have its greatest degree of reality when it stands in a position exactly corresponding to its perspective.
2. Positions in which the design arouses strong impressions of reality may be achieved by changing the angle between the plane of the design and the vertical, and the horizontal angle between the plane of the design and the frontal plane of the observer.
3. For any such design, there is probably an optimum position where its appearance of reality is strongest.

Further experiments were therefore planned to derive more detailed information relative to these two last general conclusions.

Technique B (2).

The same apparatus and the same design were used for this set of experiments as had been used for the previous set. Only the procedure was varied. The experimenter set the frame at a certain vertical angle (thus the frame was tilted forwards) and the subject, looking monocularly, was asked to adjust the horizontal angle of the frame (i.e. to turn the frame around its vertical axis) so that the character of reality of the figure should be maximum. Ten random series of adjustments for seven different vertical angles were made for the same four subjects who had participated in the preceding experiment. The subjects were also asked to describe the form of the figure when it seemed most real.

Results and Discussion of Experiment B (2).

The results for the three subjects M, K, and P followed the trend indicated in the preceding experiment; as the vertical angle of the frame was increased, the horizontal

angle was also increased to give the most satisfactory impression of reality. The average values of the adjustments of these three subjects were as follows:

	Horizontal angle		
Vertical angle	*Subject M*	*Subject K*	*Subject P*
55°	56·1°	51·1°	58·2°
50°	55·3°	51·6°	58·8°
45°	53·1°	52·6°	57·2°
40°	51·6°	49·8°	54·4°
35°	48·4°	50·6°	54·2°
30°	46·9°	49·2°	52·2°
25°	47·2°	47·9°	48·6°

On the other hand, the results of Subject B showed an opposite tendency; as the vertical angle was increased, the horizontal angle was decreased.

Vertical angle	*Horizontal angles: Subject B*
55°	35·5°
50°	34·7°
45°	37·2°
40°	41·4°
35°	43·1°
30°	44·2°
25°	48·6°

This inconsistency in the results becomes intelligible when the descriptive results are taken into account. For the three subjects M, K, and P, the figure seemed most real when it appeared to be projecting perpendicularly from the background; for Subject B, the figure was most real when the oblique lines of the figure were hanging down vertically from the background: that is, for Subject B, the form most real was that in which the projecting lines lay in fronto-parallel positions; for subjects M, K, P, the form most real was that in which the projecting lines were perpendicular to the posterior face of the figure and therefore not frontal with respect to the observer.

It seems, then, that there are two possible favourite positions in which a figure may possess a maximum character of reality. The existence of such favoured positions implies a functional relationship between the vertical and horizontal positional angles of the figure, relative to the observer; the actual relationship between the two being different for the two types of organization. For what is probably the more usual type of organization, the relationship appears to be in terms of one angle being an increasing function of the other, and this corresponds to apparent perpendicularity of the parallelepiped on its background; in the other case, the one angle seems to be a decreasing function of the other.

Technique B (3).

The other tentative conclusion which was drawn from the preliminary experiments in this group and which demanded verification, was that there is an optimum position for such a design to give the strongest impression of reality. A method for testing this conclusion was devised and the material employed consisted of five of the identical designs which had been used in the two previous experiments. These five designs were fixed, one below the other so that the distances between their centres were constant, at 19 cm.,on a wooden frame which could rotate about its vertical axis. The observer's head was held in a head-rest in such a way that the distance from his right eye to the centre of the top design was 87 cm. and the angle between his line of regard and the vertical axis of the frame was 74° 15′; so that when the frame was

rotated to the horizontal angle of 71° 20′ (90° representing as usual the frontal position of the plane) the top design was in the "correct" position, according to its perspective.

Before the experiments were made, each subject was shown a specimen drawing to indicate what was meant by apparent reality; most of the subjects had never performed such experiments and so it was necessary to start with a short demonstration to show what was to be observed and none of the experiments were started until the subjects fully realized the difference between a perspective drawing and a drawing possessing apparent reality.

The procedure of the experiment was as follows: the frame was set in a fairly oblique position and the observer was asked to look monocularly at the five designs and say which one (or ones) seemed the most real. The frame was then set at a different horizontal angle and the subject again required to judge the comparative reality of the five parallelepipeds. Five series of observations were made with each of thirteen subjects, each series consisting in the presentation in random order of the figures in the positions corresponding to the horizontal angles 71° 20′, 60°, 50°, 40°, 35°, 30°, 25°, 20°. At the end of the five series, the subject was asked to look at the top design only, set in the control position of 71° 20′ and answer the following three questions:

What is the shape of the figure?
What is the orientation of this figure with reference to the background?
What is the orientation of the posterior face of the figure with reference to the background?

The frame was then set in an oblique position and the subject asked to look only at the design which, according to his previous judgments, had the strongest appearance of reality, and the last two questions, referring to the orientation of the figure itself and of its posterior face, were repeated.

Results and Discussion of Experiment B (3).

The percentage results in favour of each design are given in the following table. The designs are numbered 1–5 according to their position on the frame, No. 1 being the top one and No. 5 the lowest one; associated with their positional number is the "vertical" angle between the line of regard, directed at the centre of the appropriate design, and the vertical axis of the frame. If two or more designs were considered equally real, both preferences were noted: hence the number of judgments for any position of the frame may exceed the total number of series of observations (65). If all the designs were seen as equally real, or unreal, this response was classed as Doubtful(?).

Horiz. angle	*Design number* / *Vertical angle*	1 74° 15′	2 63° 6′	3 53° 43′	4 46° 9′	5 40° 6′	?	*Total number of judgments*
71° 20′		25·3	34·1	11	8·8	9·9	11	91
60°		14·1	31·8	24·7	15·3	1·2	2·3	85
50°		14·5	20·5	30·1	22·9	10·9	1·2	83
40°		7·5	10·8	30·1	36·6	15·1	—	93
35°		7·5	4·3	29	41·9	16·1	1·1	93
30°		9·2	3·9	19·7	**52·6**	14·5	—	76
25°		6·7	2·2	16·8	51·7	21·3	1·1	89
20°		7·9	5·7	13·9	45·5	27·3	—	88

The trend of the results is similar to that of the other experiments, showing that there is some kind of functional relationship between the vertical and horizontal angles of presentation. But it is also apparent that there is an optimum position for this design, i.e. at a horizontal angle of about 30° and a vertical angle of about 46°. These quantitative results are borne out by the descriptive results; although most of the observers could pick out, for any angle, one or two designs as being more real than the others, the designs which were picked out as the most real at the large horizontal angles did not look as real as those picked out as being most real at the more oblique angles of presentation. Even those subjects who found difficulty in distinguishing different degrees of reality between the designs at any given horizontal angle, agreed that they had more reality for the more oblique positions of the frame.

The responses to the questions regarding the shape and orientation of the figures were very consistent. For the design in its "correct" position, all the subjects saw the figure as a cube or parallelepiped, oriented obliquely to the background, but in almost all cases the posterior face itself was seen to be in the same plane as the background. Occasionally, however, this face was seen to be oblique to the ground in the same way as the figure as a whole. But the greater number of subjects gave the apparently contradictory responses which imply that the posterior face of the figure is situated in two different planes at the same time. When the questions were repeated in reference to the design when it was in the position associated with the strongest impression of reality, the figure was seen to be perpendicular to the background by all but one of the subjects. Once more, the form of the figure was no longer that of an elongated parallelepiped, or even a cube, but had the shape of a very flat parallelepiped.

Following this, the five designs were replaced by five other identical designs made from the second of the photographic slides (the angular values 69° and 81°); exactly the same apparatus and procedure were used with five subjects and the percentage results, shown in the table below, completely support the results of the former investigation. The functional relationship between vertical and horizontal angles is again apparent and the existence of an optimum position is clear, though this position is evidently different from the optimum position for the first design.

Horiz. angle / *Design number* / *Vertical angle*	1 74° 15′	2 63° 6′	3 53° 43′	4 46° 9′	5 40° 6′	?	*Total number of judgments*
71° 20′	53·1	31·2	6·2	—	—	9	32
60°	59·4	34·4	6·2	—	—	—	32
50°	47·1	41·2	11·8	—	—	—	34
40°	27·6	65·5	6·9	—	—	—	29
35°	20·7	75·8	3·4	—	—	—	29
30°	13·3	80	6·7	—	—	—	30
25°	10	83·3	6·7	—	—	—	30
20°	—	**100**	—	—	—	—	25

The angles (71° 20′ and 74° 15′) which corresponded to the exact perspective of the first design did not correspond to the exact perspective of the second design, but were used in order to have exactly the same experimental conditions.

V

General Conclusion

The most important conclusion to be drawn from the Group B experiments is that a simple perspective drawing is not most real when it is designed in such a way as to be perspectively correct from the point of view of the observer; though it is perhaps in such a case that there is the greatest degree of likeness between the shape seen in the picture and the shape of the represented real object. This is certainly true for ordinary pictures, generally speaking. But it is not under these conditions that one has the strongest impression of apparent reality of extension in the third dimension. The results of the experiments also indicate the probable reason why this is so, and consequently the conditions under which a given design will acquire the character of reality. There is a conflict in the perception of a perspective drawing when it is looked at from a normal point of view. In the particular figure we have been using, the conflict arises because the parallelepiped is normally seen as oriented obliquely to the background; i.e. the oblique lines of the figure do not seem to be perpendicular to the background. But these lines are seen to be perpendicular to the posterior face of the figure. Yet the figure is so placed on the paper that this posterior face appears to be located in the plane of the paper and if this were true then the oblique lines should seem to be perpendicular to the paper, which is evidently not true. The conditions, then, are such that the figure cannot be satisfactorily seen as a real form oriented either obliquely or perpendicularly on the background. The conflict arising from these two opposing organizations is resolved by the loss of reality of the figure which is therefore seen as a two-dimensional representation of a solid object.

Therefore one would suppose that if the objective conditions were arranged in such a way that no such conflict would arise, then the figure would have a compelling appearance of reality. In other words, the figure would become real if the observer saw the oblique lines of the parallelepiped as perpendicular to both the posterior face of the figure and to the plane of the paper. And this is the case when the drawing is looked at monocularly from an oblique point of view, as has been shown by our experiments; but in that case, there is a distortion of the apparent shape of the object. In fact, in our experiments, the parallelepiped looked much flatter when it seemed most real than it did when seen as a perspective drawing in the frontal plane.

For a few subjects, the design has always a certain degree of reality even when it is shown in the control position. In these cases, the posterior face of the figure is seen as being oriented obliquely to the background in the same way as the whole figure—in other words, the freedom of the figure from the ground is so strong for these subjects that the usual conflict does not arise and consequently there is no loss of reality. But even for these subjects, the impression of reality is stronger when all the objective conditions are tending to produce the same perceptual organization. It is perhaps possible to train subjects in this direction, to obtain segregation between figure and ground, even when looking at the picture from the exact point of view.

As to the reason why the obliquity results in apparent reality of the third dimension, the two sets of experiments give relevant information. Results have shown that there is a preliminary condition—namely, that integrating factors, tending to bind the drawing to the plane of the paper, should be reduced to a minimum, and that

segrating factors should be strengthened. We have seen that the integrating factors were mostly factors of similarity, i.e.:—

1. Similarity of size of figure and size of ground.
2. Similarity of perspective clues arising when the distance between subject and figure is small.
3. Binocular parallax.
4. Similarity of brightness of figure and ground.
5. Similarity of microstructure of figure and ground.
6. Similarity of perspective distortion of design and support.

When these integrating factors are reduced, there is a possibility of the figure becoming segregated from the plane. But to actually achieve the segregation and the organization in the third dimension, our experiments have shown that another factor must operate. In our experimental conditions, the organization in the third dimension becomes more real when the orientations of the lines and the background are such that the object may seem to extend *perpendicularly* from the surface of the ground. This seems to be a pregnant form of organization which arises as soon as the pattern of stimulation allows it.

Throughout this report, we have been speaking of the freeing of the figure in a purely introspective way. A stroboscopic experiment was therefore devised to demonstrate the freeing in a more evident manner; the demonstration was found to be quite coercive, the conditions being such that all the relevant factors were co-operating to produce the same impression. Of the two figures used in the stroboscope, the first consisted of a 3·2 cm. horizontal black line, width 1 mm., whose proximal end was enclosed by a red contour square of 1 cm. side; the centre of the square was not exactly at the tip of the line, but was displaced 2 mm. along it, so that when the figure was looked at from an angle of about 15°, the tip of the line appeared to be located at the centre of the square. The second figure consisted of a short black line of length 8 mm. and width 1 mm., drawn so that its tip was at the centre of a red contour square of 1 cm. side. Both figures were drawn on similar white cards, 10 cm. × 6 cm. As these cards were set in the stroboscope, the observer saw alternately the first figure at an angle of about 15° (between the line of regard and the plane of the card) and the second figure in a frontal position (90°). Hence, for each of these views, the central black line had the same stimulus length of 8 mm. while the size and orientation of the red square and white card changed. The apparatus was set so that the observer, looking monocularly, was presented with the two views in rapid succession in such a way that the image thrown on the retina by the black lines remained exactly the same. This resulted in the compelling impression that the white card and red square base turned from a frontal position to a very oblique position while the black line retained its independence and orientation in the frontal plane, and was linked with the plane only at the point where it touched it in the centre of the red square. To produce this compelling impression it is essential that all the factors are working in the same sense; for instance, it was found that there was a certain amount of ambiguity in the impression if the line and the square were both drawn in black. This similarity in brightness and colour of the two parts, one of which tends to be free and the other of which tends to remain in the background, completely destroys the illusion for some observers.

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References

1. Franklin, Ladd (1887). Method for experimental determination of the horopter. *Amer. J. Psychol.* **1**, 98–111.
2. Franklin, Ladd (1896). An optical illusion. *Science*, **3**, 274–5.
3. Michotte, A. (1948). L'énigme psychologique de la perception dans le dessin linéaire. *Bull. Acad. Belg. Cl. lettres, 5e Serie.*, **34**, 268–88.
4. Sanford, E. C. (1898). *Experimental Psychology.*

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LEARNING AND UNDERSTANDING

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Short texts containing rather difficult pieces of logical argument were presented, sometimes with illustrative graphs and sometimes without, to girls from the sixth form of a grammar school. They were required to study these, and recall afterwards what they were about. It was found that frequently there was a comparatively accurate recall of many of the individual items of the texts, but that certain essential points in the argument were omitted. Thus learning of detail could occur when there was no general understanding of the meaning. The effect of the illustrative graphs was to make the recalls even less coherent.

I

Introduction

To conclude investigations* of the ability to understand information, presented graphically, it was decided to discover if information was derived more readily from study of a brief written text illustrated by graphs, or from a written text without such illustration. In the course of this inquiry, there appeared some rather startling evidence of the inability to understand the meaning of the material presented. In this paper are discussed some of the results of this inability, and the conclusions which may be drawn with regard to the effect of learning by rote upon learning by understanding of meaning.

II

Method of Experiment

The material used in the experiment consisted of two short written texts, each of about 400 words. The first dealt with recent changes in the sizes of populations, and the relation of these changes to fluctuations in the birth and death rates. The second outlined the relation of disease and death to poverty. Graphs were prepared which showed the main data upon which the arguments of the written texts were based. There were five graphs for each text, and they were interleaved with the text so that each graph appeared on the page on which was presented the corresponding verbal information.

There were two groups each of 11 subjects, who were girls aged 16–18 from the sixth form of a grammar school. The first group was given Text I illustrated by graphs and Text II without graphs; and the second group was given Text I without graphs and Text II with graphs. The subjects were tested individually. Each one studied Text I for ten minutes, and then endeavoured to recall as much of it as possible, orally; the recall was recorded verbatim. Text II was then treated in the same way.

The recalled material was scored in two ways: First, one mark was given for each individual item of information remembered (only items of some significance were

* An earlier investigation into the understanding of material presented graphically was carried out by the author while a member of the scientific staff of the Medical Research Council. A brief account of this is given in the *British Journal of Psychology*, 1946, **36**, 146–158. A report on a further investigation will be published shortly in the *British Journal of Educational Psychology*.

scored—linking sentences were omitted). But there were also certain general statements or groups of items the omission of which showed that some essential part of the argument had not been fully grasped and remembered. There were seven of these key points in each text; and the number of these omitted was scored separately.

III

Results

1. *Effects of Difficulty of the Material.*

As in previous investigations, it was found that the material of Text I was considerably harder to understand than that of Text II. The number of individual items recalled was 45 per cent. of the total for Text I, and 46 per cent. for Text II; the difference was not significant ($P = 0{\cdot}78$). However, 59 per cent. of the general statements of Text I were omitted, and 30 per cent. of those of Text II; the difference was highly significant ($P < {\cdot}01$).

When the recalls were examined for qualitative differences, the effect was even more apparent. The original of Text II consisted mainly of a succession of fairly straightforward statements such as, that adult and infant mortality were higher in the lower than in the upper social classes; that certain diseases showed this effect particularly clearly; and that certain conditions of livelihood inherent in poverty appeared to be responsible for these effects. These statements could be separately assimilated, and if they were not recalled in exactly the right order, the general gist of the passage was not greatly impaired.

In Text I, on the other hand, the logical succession and inter-relations of the statements presented in the text was much more marked. The most important statements were that the population of Great Britain had increased up till the present day, whereas the birth rate between 1880 and 1940 had steadily decreased. These at first sight contradictory effects were reconciled by the stated fact that increase of population occurs as long as the birth rate exceeds the death rate, which it had done and still is doing. Now one subject only gave this essential relating statement in her recall. Thus it was apparent that the other subjects did not really grasp the general argument; and indeed, their recalls were full of inconsistencies and errors. The most frequent mistake was a confusion between size of population and birth rate. Thus it was said in one place that the birth rate of Great Britain had decreased, and in another place that it had increased (whereas it was the population that had increased). And it may be, of course, that the difficulty in grasping the essential point noted above was due to the fact that size of population and birth rate were never clearly differentiated by the subjects.

Whatever the source of the difficulty, the fact remained that the omission of a single key point prevented the subjects from really understanding the general argument of the passage as a whole. What appeared to remain in their minds was a jumble of ideas about population, with few if any clear ideas of what was happening to it, and why. But we noted above that the percentage of individual items recalled was not significantly less in Text I than in Text II. Hence it appears that an individual is capable of reproducing quite a number of isolated facts, but they may be jumbled up, without any obvious inter-relationship, and without leading to any coherent meaning.

This is not to say that even the individual items remained intact in the general confusion. A further consequence of the lack of understanding of the general meaning of the passage was that words and phrases were reproduced often with only

slight changes from the original; but the changes were such that they might destroy the true meaning of the phrases, as in the following examples:—

"There was at one time an increase in birth rate, but only very little over the death rate because there were more people dying." "The birth rate slowly increased owing to people living longer." "In 1940 the birth rate increased slightly, so that there was a small increase of population but only slight; so the population may again be stationary and decrease." "In 1880–1941 the birth rate seemed higher because the number of births had been comparatively more than the death rate of the people each year."

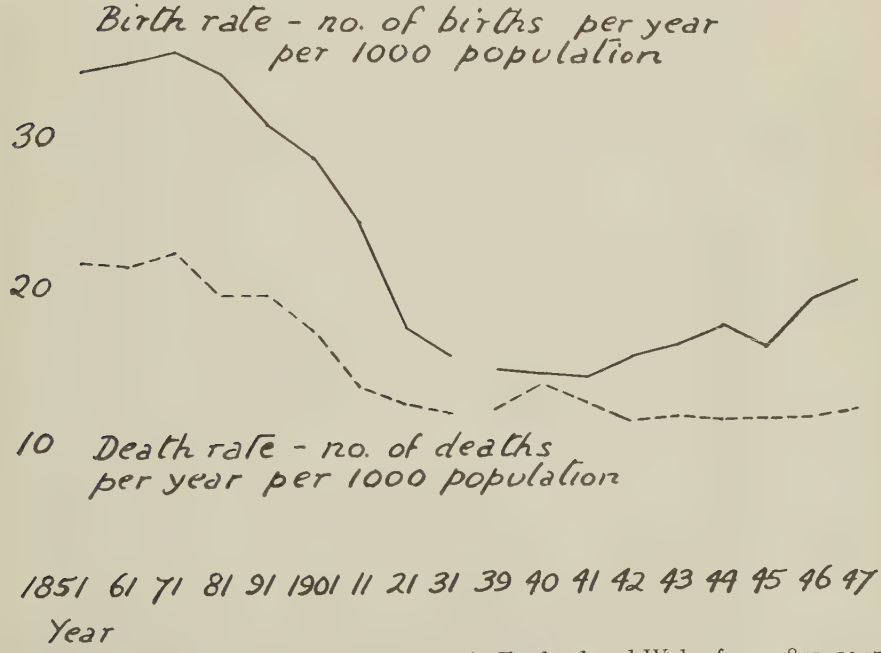
Again, when certain important items were omitted in the spontaneous recall, they could often be elicited from the subject by asking appropriate questions. In other words, this information had been retained by the subject, but it was unattached to the remainder of the information by a coherent scheme of relationship. Therefore it was not spontaneously available. Thus a subject cannot make efficient use of information which cannot be "fitted in" to some general argument.

2. *Effects of Graphical Illustration.*

Before passing to a more general discussion of the implications of these results, a few comments may be made on the effect of the graphs. In general, they seemed to decrease the accuracy and coherence of the recall. The number of individual items recalled was 37 per cent. of the total, when there were graphs; and 54 per cent. of the total when there were no graphs. The difference was significant ($P < \cdot 01$). In the

FIGURE 1

Graph Illustrating Text I.


Birth rate - no. of births per year per 1000 population
30
20
10
Death rate - no. of deaths per year per 1000 population
1851 61 71 81 91 1901 11 21 31 39 40 41 42 43 44 45 46 47
Year

The trends of the birth-rate and death-rate in England and Wales from 1851–1947.

first case, 51 per cent. of the more general and essential statements were omitted, and in the second case, 38 per cent., the difference being significant ($P = 0.048$).

From a general examination of the recalls it appeared that the subjects lacked the ability to integrate the specific facts presented in the graphs with the statements made in the text. Sometimes the graphs were ignored completely, after a cursory inspection. Sometimes the subject spent so long and became so confused in trying to incorporate facts from the graphs that essential points from the text were omitted. And sometimes a description of the graph would be suddenly introduced in the wrong place, or produced as an afterthought at the end of the recall. Thus one subject, having completed a recall of Text I which more or less followed the text, added at the end:—

> "The death rate decreased till 1939, then shot up during 1940–41, but has been decreasing since then. The birth rate decreased till 1941, rose slightly, then it's been decreasing gradually" (see Figure 1).

In general, the recalls of material from the written text alone were better arranged, smoother and more coherent than those of the text with graphs. In certain circumstances this tendency of the graphs to break up the continuity of the written text might serve the useful purpose of preventing the slavish acceptance and copying of the statements in the text. But this effect would be valuable only if the subject were able, first to assimilate the details of the graphs; secondly, to check the written statements against them; and thirdly, to integrate the whole into a coherent logical argument. It did not seem that the subjects of this experiment, at least in the time at their disposal, were able to perform this task.

IV

Conclusions

We reach then the conclusion that more or less accurate memorization of particular words and phrases, and even of whole statements, can be achieved without any assimilation of the general argument or understanding of the ultimate meaning of a written passage. Indeed, it appears almost as if the effort to memorize detail accurately may preclude a more general understanding. But this seems a contradiction of the "effort after meaning" tendency. What is more probable is that if the meaning can be obtained only with considerable effort, and if the subject lacks the ability and the interest to make this effort, he is liable to fall back on the recall of isolated items, as if to save a few remnants from the general confusion.

Certain consequences arise from this. In the first place, we see that partially assimilated information may be a ready source of inaccurate rumours and tendentious opinions. Thus, there was a sentence in Text II which stated: "Tuberculosis, which is particularly fatal to young women, has a close relationship to wages." This was reproduced more than once as: "Tuberculosis is fatal to young women." It so happened that these girls were about to undergo mass radiography. The implications are obvious! We can understand how quite sensible statements in a newspaper, when isolated from a meaningful context, may be distorted by the displacement of a few words to produce wholly erroneous impressions.

Secondly, it seems useless to test a subject's real understanding and recollection of a connected prose passage by scoring the number of isolated items remembered. Such items will be useless as they stand, and are indeed unlikely to persist in their original form.

Lastly, these results throw considerable doubt upon quantitative theories of earning. The efficient learning of a meaningful task may be almost in inverse roportion to the number of individual items learnt, in particular, of isolated words nd phrases. For if the subjects could coherently reformulate in their own words he general arguments of the text, one could be fairly certain that the arguments ad been grasped. If, on the other hand, the words of the text were slavishly opied, one would be much more doubtful. Accurate reproduction of detail is not ecessarily inconsistent with memorization of a general argument, but it does not nsure it, and may, indeed, make it more difficult.

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THE DIRECTION OF MOVEMENT OF MACHINE CONTROLS

BY

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Experiments are described in which the relation between the direction of movement of a control and display was varied in order to determine what relationships are likely to cause least confusion to the operator of a machine. Two different types of task were used: a task consisting of intermittent stimuli whose rate of presentation could be varied, and a continuous pursuit task.

It is found that accuracy of performance varies with the degree of remoteness of the directional relationship from those met in everyday life, with the complexity of the task, with the ability of the subject, and there is some indication that under certain conditions it may vary with the breadth of his attention. Awareness of the response aspect of the situation in these simple sensori-motor tasks, it is suggested, although necessary in the early stages of learning, may be associated in the later stages with confusion and error.

I

Introduction

In many mechanical skills met with in industry and everyday life, the operator of a machine is required to produce movements of one part of the machine (the "display") by making directional movements with a control, such as a knob, lever or handwheel. One simple aspect of the problem of machine design is thus concerned with the question of what relationships between the direction of movement of the control and that of the display are most quickly and effectively learned. It is generally recognized that an "expected" directional relationship—one in which the display and control move in the same direction—will be the easiest to learn, and will be least likely to cause error or accidents. The "expected" relationship, however, may not always be immediately practicable for the designer, and in this case it is important to know whether an "unexpected" relationship, in which display and control move in opposite directions, or an ambiguous relationship, will cause more confusion to the operator of the machine.

Many experiments have been concerned with this problem, and the stage seems to have been reached where general conclusions can be drawn from them. For this purpose a number of these experiments have been summarized below. The results appear to indicate that an untrained operator is likely to show real preferences for certain directional relationships and, moreover, that under certain conditions preferred directional relationships are reflected in performance, in a serial task. The effect of an "unexpected" or ambiguous relationship may not be felt, however, unless the task is one where the required response rate is fairly high, or the task itself fairly complex, or unfamiliar as when performed by the left hand. It is suggested that the confusion resulting under these conditions may be produced by forcing the operator to direct his attention to the response aspect of the situation, as awareness of the response appears to be associated with difficulty and error.

It is not known whether intensive training will remove the hampering effect of an "unexpected" relationship in a serial task, but such a task seems to be difficult to learn under certain conditions, especially for subjects with low intellectual ability.

II

Preferred Directional Relationships

In the handling of machine controls it is known that certain directional relationships will be "expected," or regarded as "natural" by the majority of operators, while other relationships may be "unexpected" or seem "unnatural." There is, in addition, some experimental evidence to show what these preferred relationships are when subjects are merely asked to move a control in what they think will be the appropriate direction to bring about a specified movement of a display. Warrick (reported by Fitts 1947) has shown that when rotary movements of a control knob are required, and where indicator and control move in the same plane, the operator usually moves the control so that the part of it nearest the indicator moves in the direction in which he is attempting to move the indicator, but where the control knob does not move in the same plane as the indicator the response tendencies are ambiguous. These results have been confirmed by Gibbs (1950) in the operation of actual machine tools using a handwheel.

Similarly, using levers and pointers moving at right angles to each other and considering each hand separately, Mitchell (1948) found that a large majority of right-handed subjects favoured movements inwards to move the pointer up (right hand to the left or clockwise, and left hand to the right or anti-clockwise), and the opposite movements, outwards (right hand to the left, and left hand to the right) to move the pointer down.

These results, which show a general meaningful trend, although they do not give a complete picture, are summarized in Table I.

TABLE I

Preferred Directional Relationships

	Display moves up and down, control on right-hand side of display: % *giving clockwise movement for up and vice versa*	*Display moves up and down, control on left-hand side of display:* % *giving anticlockwise movement for up and vice versa*	*Display moves to right and left.* % *giving clockwise movement for right and vice versa*	*Display moves backwards and forwards, control with axis parallel to operator.* % *giving clockwise movement for towards operator and vice versa*	*Display moves backwards and forwards, control with axis at right-angles to operator.* % *giving clockwise movement for away from operator and vice versa*
Warrick (knob) (Fitts 1947).	60	57	77	27	30
Gibbs (handwheel) (1950).	89·3	—	93	43	46
Mitchell (lever) (1948).	78 (right hand)	78 (left hand)	—	—	—

Note: Warrick's figures generally give lower percentages as his group contained a number of subjects who were inconsistent. Gibbs and Mitchell gave subjects only one try. Warrick's figures are probably the most reliable therefore, but the same trend is shown in all.

III

The Effect of Directional Relationship on Performance

a. Discontinuous Serial Tasks.

In addition to the evidence of preference for certain directional relationships, there is evidence that under certain circumstances some combinations of direction that are "unexpected" when incorporated into a serial task, give rise to an initially poorer performance than other, "expected" relationships.

There are obvious reasons for this; it can be demonstrated that if a stimulus (e.g. an indicator that requires to be moved *up*) calls for a response that is not the one that would be the first to be thought of (an upward control movement) the time lag between stimulus and response will be longer than if an "expected" response movement were required. It has, further, been shown (Vince 1950) that the reaction time for an "unexpected" response even after a long period of practice is longer than that for an "expected" response. The reaction times in this case were 0·419 compared with 0·359 second. Performance which requires a high response rate should therefore be affected adversely by an "unexpected" control-display relationship.

An experiment showing that this is the case, is described by Vince (1945). In this experiment (which will subsequently be referred to as the "discontinuous task") the stimuli were separate ink marks either above or below a horizontal line on a moving band of paper, and subjects responded to each mark by moving a control knob in the "expected," or same, or in the "unexpected," or opposite, direction. The knob had an all-or-none effect, and actuated a pencil marker, so that subjects had immediate knowledge of results. Results show that when stimuli are separated by an interval of four seconds the difference between "expected" and "unexpected" connections is slight, that when the interstimulus interval is two seconds, subjects made about twice as many errors with the "unexpected" as with the "expected" connections, and that at faster rates, the difference between the two becomes even greater; what happens is that performance with the "expected" connection shows little deterioration up to an interstimulus interval of 0·5 second, and then falls off, while with the "unexpected" connection, performance falls off at intervals shorter than one second, and then more rapidly.

A further question which arises from the problem is whether more apparently ambiguous relationships, where preferences are less unanimous, are easier, or more difficult to grasp and learn than the more obviously "expected" and "unexpected" relationships. Using a similar task, but with stimuli spaced irregularly, and appearing at a mean rate of 1·4 per second, Vince and Mitchell (1946) were able to place six directional relationships in an order of accuracy of response, as shown in Table II. Over the initial learning period which was studied, this order seemed fairly stable, and in it directional relationships in different planes were placed between the "expected" and "unexpected" relationships.

The stability of these results was tested by giving the subjects an extra task to carry out with the left hand (Mitchell 1947). Here the previous experiment was repeated, but at the same time subjects operated a handwheel at a constant rate, winding anti-clockwise, with the left hand. The effect on the right-hand task seems to have been to exaggerate the confusion caused by difficult relationships, while preserving, in the main, the rank order of accuracy.

Here again, the rank order of accuracy, and scores, obtained in this experiment are given in Table II. In both experiments using the discontinuous task, the differences in error score between the various directional relationships were mostly large enough to be statistically significant.

TABLE II

EFFECT OF DISPLAY-CONTROL RELATIONSHIP ON PERFORMANCE

(Results from Vince and Mitchell 1946, Mitchell 1947, 1948 and 1949)

Discontinuous task		*Continuous task*			
				Right hand in 2-handed task:	
One hand	*Two hands*	*Left hand only*	*Right hand only*	*Left hand = F*	*Right hand = B*
Error score—mean of 5 runs	*Error score—mean of 5 runs*	*4th Run error score*	*4th Run error score*	*Error score*	*Error score*
U 2·98	U 4·16	U 2·51	L 2·67	F 1·91	U 1·89
F 4·52	F 5·3	R 2·73	U 2·86	U 1·93	L 2·03
R 6·6	— —	B 3·04	R 3·04	L 2·31	D 2·13
B 7·0	L 9·2	F 3·32	B 3·14	R 2·51	B 2·53
L 7·4	D 11·12	D 3·32	F 3·25	D 2·73	R 2·67
D 7·98	B 11·88	L 3·79	D 3·54	B 3·72	F 2·79

"U" = control movement up to move pointer up, and down for down.
"F" = " " forward " " up, " backward "
"R" = " " right " " up, " left "
"B" = " " backward " up, " forward "
"L" = " " left " " up, " right "
"D" = " " down " " up, " up "

The effect of directional relationship on performance appears to be fairly marked in the discontinuous task, and the resulting rank order relatively stable. With the continuous task differences are very slight when the right hand is used, and a little more marked when the left hand is used; here again there is some suggestion of a similar rank order. In the two-handed task the rank order might be considered as produced by the combined effect of the practised left hand relationship and the "naturalness" or otherwise of the right hand relationship. As these results are taken from six different groups of subjects, *inter* group comparisons are likely to be misleading.

b. Continuous Pursuit Tasks.

A discontinuous serial task was chosen for the experiments previously described, as it was the simplest that could be envisaged for this investigation and could be varied in a controlled way for speed and directional relationship. As a task, however, it is not particularly related to the conditions of ordinary life, where rapid series of discrete movements are not often required. For this reason the results obtained previously were checked using a simple pursuit task. The chief differences between this and the preceding task being that now (i) the stimulus is changing continuously, so that for certain periods of time, the rate and direction of movement can be anticipated, also (ii) that knowledge of results is forced on the subject, as the misalignment depends on his previous performance as well as the cam-controlled pointer movements, and (iii) that the response rate is determined by the operator, although the size of misalignment that he has to correct depends on the rate at which the stimulus is moving as well as on the accuracy of his immediately preceding performance. In

this experiment the task for the subject was to keep a moving pointer on a horizontal line, by moving a handle up or down. The pointer was moving continuously up or down, at the fairly slow speed of approximately 0·4 in. per second. As before, the connection between handle and pointer could be varied in the same six different ways.

Under these conditions, when right-handed subjects performed the task with the right hand there were no statistically significant differences between error scores obtained with the six different directional relationships used, although there were small differences giving a rank order of accuracy which is given in Table II with the scores (Mitchell 1948). This confirmed the finding of Grether (1947) that, when a pointer with an irregular oscillating motion had to be maintained over a reference mark by means of a rudder pedal or stick control, directions of control and display had little effect on the accuracy with which the unpractised subject performed a short one-dimensional tracking task. That the directional relationship is of little importance in a simple one-dimensional tracking task was shown by Vince (1945). Here it was found that when the relationship between control and display was reversed between two trials without warning, half of the subjects continued to track as accurately as before and did not notice the change. Whereas the other half did notice and their performance deteriorated, but this feature of the results will be discussed later in Section 6.

It seems then that in a simple continuous pursuit task where the preferred hand is being used, the direction of movement of the control in relation to that of the display is not an important element of the performance, and that conscious awareness of this relation is not necessary for efficient tracking.

A tentative explanation of this difference between the effects of the directional relationship on discontinuous and continuous tasks has been found in the operator's tendency to oscillate in the continuous task; in the early stages of learning he may correct a misalignment by overshooting the target, this corrective movement being followed by a second overshoot in the opposite direction. Successful tracking could therefore be carried on by the trial and error method of making alternate movements in opposite directions. This method would require little cognitive appreciation of the connection between control and display. An alternative method was to lag behind the target and put in a series of corrective movements in the same direction, until the direction of the target motion itself changed. Again, when errors of direction did occur they could be rapidly appreciated and corrected before a large misalignment developed, and might not always be detectable in the records, although efforts were made to assess them.

It is possible, however, that a more fundamental explanation might be found in a factor which forms an integral part of this continuous task and is less important in the other: that of knowledge of results. As stated above, the fact that in this task a misalignment is the algebraic sum of the target course and the operator's response presents the subject with knowledge of results in a particularly emphatic form: it is not only what he has done, but what he has to do. It is this factor which makes it possible to call the continuous task a skill according to the definition of Bartlett (1947b); it has pre-eminently the "character of being in touch with demands which come from the outside world." Faced continually with the information of how well he is responding to these demands relieves the subject perhaps of the necessity of turning his attention to the response.

A one-dimensional tracking task differs then, from the discontinuous task in that the directional relationship is not an important factor in performance.

A slightly different picture emerges, however, when tracking results for the left and non-preferred hand are considered (Mitchell 1948). Here an order of accuracy

emerged, and is given in Table II, and here some of the differences (those between the best and two worst, and the worst and two best relationships) are statistically significant. It is of interest to note that where handle movements towards the right and left are concerned both for right and left hands the best performances were obtained with those relationships which were found to be the preferred ones (see Section 3), although this did not hold in the discontinuous task where differences between performance with right and left response movements for an upward movement of the display were slight.

Again, when a tracking task is performed with both hands simultaneously, statistically significant differences appear between different directional relationships (Mitchell 1949). Here subjects watched two pointers, each of which was moved independently by a cam, and also by a handle, operated by the subject, one for each hand. Two different conditions were employed. The two groups of subjects were given the task in six sessions, in each of which the directional relationship for the right hand was different, while that for the left hand remained constant throughout, being for the first group a movement of the control handle forwards, to move the pointer up, and for the second group the left hand task was reversed, so that a backward movement of the control was required to move the left hand pointer up throughout. The results given in Table II suggest that the rank orders of accuracy obtained in this way were affected by the most familiar aspect of the situation; the directional relationship employed by the left hand. This seems to have dominated the mental set of the subjects and rendered more difficult otherwise "easy" relationships for the right hand. It is, for example, interesting to note that whereas when the left hand relationship was a forwards movement to move the pointer up, that relationship was also the best for the right hand while the opposite relationship, a backwards movement to move the pointer up, was the worst and this was significantly different from all the others. (Also in this experiment the two best, and two worst relationships were significantly different from each other.) Again, when the left hand relationship was reversed, for the second group to a backward control movement to move the pointer up, the greatest amount of error was made when the relationship for the right hand was a forwards movement to bring the pointer up. However, in this part of the experiment the backwards movement for the right hand occupies its approximately normal position in the rank order, and the only difference which was statistically significant was that between the best and worst relationships.

The main point brought out by this experiment is that when the situation is made more complex, performance again appears to be affected by the directional relationship between control and display.

Similarly, in a rather complicated display-control relationship problem which seems to have contained some elements similar to both the continuous and discontinuous tasks, Carter and Murray (reported by Fitts, 1947) found that the relationship between the axis of movement of the control and the line of movement of the display was important and also that the relationship between the direction of movement of the display and of the control was important for reducing errors and time for performance. In their words:—

> "It would seem that, if the display and response required are very simple and if considerable time is allowed for responding, the display-control relationship may not be important, but if the display or response required is complex or if the operator is hurried, the display-control relationships are of extreme importance."

Some further evidence which points to the same kind of conclusion is provided by Craik (1944), where constant rates of handle winding had to be maintained by

right and left hands separately. When the rate of winding was slow (10 r.p.m.), right and left handed performances were approximately similar, but when the rate of winding was increased to 100 r.p.m. larger winding errors were made by the left hand than the right, although these differences were not statistically significant.

A continuous pursuit task where the response consisted of rotary movements of a handwheel is also described by Gibbs (1950) and he found that there was a considerable difference between initial performance and also time required to learn to a certain criterion of accuracy between the "expected" and "unexpected" relationships. To obtain a perfect record his subjects would have had to wind at a rate of 18 r.p.m., so that their required rate of movement was approximately 2·8 in. per second. Here also the directional relationships were more complex as a clockwise pointer motion had to be balanced by clockwise or anticlockwise handwheel movements, and the task more complex as subjects had to superimpose corrective movements on this high basic winding rate, which must itself need adjustment from time to time. There are, then, two essential differences between these winding tasks and the continuous task of Vince and Mitchell described above: complexity of directional relationship and speed of the course to be followed. It is shown in the experiments mentioned above that an increase in the complexity of the task will result in a difference between the "expected" and "unexpected" relationships even in this continuous task, and it is possible that a considerable increase in speed might have had the same effect. The latter, however, is an hypothesis which remains to be tested.

IV

Learning and Individual Differences

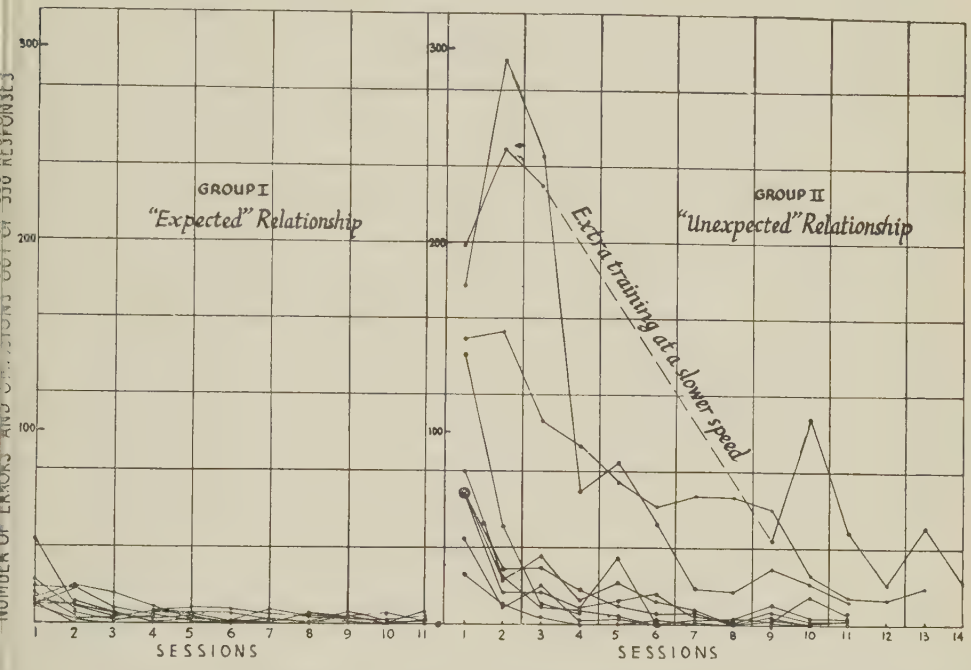
An attempt was made by Vince (1950) to compare rates of learning, and also retention under disturbed test conditions with the "expected" and "unexpected" directional relationships, in a discontinuous task where subjects were not given visual knowledge of results. There were two matched groups of subjects. The "unexpected" group on the average achieved a lower level of performance at the end of the learning period, than the "expected" group. Their retention of the skill under disturbed conditions was also inferior to the "expected" group. There is also a suggestion in this experiment that, in the same way as increasing the complexity of the task exaggerates the difference between performance with the different directional relationships, so these more difficult tasks also exaggerate the differences between subjects, on the basis of their initial performance and rate of learning. It was pointed out by Vince (1945) that although the difference between errors made with the "expected" and "unexpected" discontinuous tasks was statistically significant, nevertheless, there was much overlap between scores obtained on these two tasks: the "expected" connection affected a few subjects to a marked degree and most very little. The same kind of picture emerged in the learning experiment. Here differences between subjects who carried out the task using the "expected" connection were slight, but the other group of subjects doing the same task with the display-control relationship reversed, varied considerably both as regards their initial performance and their rate of learning (Figure 1). These individual differences were in the main preserved throughout the learning period, and there was a suggestion that ability to perform this "unexpected" task correlates with intelligence, as might be expected, considering that the cognitive element in such a task is relatively dominant.

FIGURE 1

Learning and Individual Differences with "Expected" and "Unexpected" Directional Relationships.

Individual Learning Curves.

(Results from Vince, 1950.)



ndividual learning curves show that an "unexpected" directional relationship not only ffects initial performance and rate of learning as compared with an "expected" relation-hip, but differentiates between subjects to a marked degree. In this experiment subjects were not given immediate visual knowledge of results.

V

Role of Awareness of the Response

The preceding results suggest that the differences between performance with various display-control relationships are associated with the amount of conscious understanding which is required for their performance, or the degree to which the relationship is removed from those practised in daily life.

In this type of task there is, of course, a high conscious appreciation of the *end* for which the task is performed; the subject is concerned with what he has to do and how successfully he is doing it. But the preceding experiments tend to indicate that where the task is less familiar and more complex the *response* itself, as well as the result of the response must be included in the subject's field of attention.

In order to investigate this problem to some extent a number of experiments were devised where the connection between control and display was reversed in the course of the experiment, thus reversing the direction of the required hand movements. This was done mostly between two trials, and always without warning the subject. It was discovered that in many cases this reversal could be made without

being noticed by the subject, and sometimes without affecting his performance to any marked degree. Results were assessed by dividing the subjects into three groups, those who noticed the reversal, those who did not notice, and those who were

TABLE III

EFFECT ON PERFORMANCE OF AN UNEXPECTED REVERSAL OF DIRECTIONAL RELATIONSHIP

(Results from Vince (1945) and Mitchell (1946))

1. DISCONTINUOUS TASK

(1) *Fast rate*		*Uncertain*	*i. Before reversal*				*ii. After reversal*			
			Noticed		*Did not notice*		*Noticed*		*Did not notice*	
Directional Relationship										
Group	*Before reversal*	*After reversal*	*Number of subjects*	*Score*	*No. of subjects*	*Score*	*No. of subjects*	*Score*	*No. of subjects*	*Score*	*No. of subjects*
(*a*)	Forwards	Backwards	3	4·3	15	4·6	7	8·2	15	6·1	7
(*b*)	Backwards	Forwards	6	6·6	10	8·0	9	8·4	10	8·6	9
(*c*)	Right or Left	Left or Right	13	5·4	19	8·1	18	7·6	19	5·6	18
	(2) *Slow rate*										
(*d*)	Up	Down	—	0·8	5	—	—	6·8	5	—	—
(*e*)	Down	Up	—	0·4	5	—	—	4·8	5	—	—

2. CONTINUOUS TASK

Directional relationship		*Mean error score*											
		3 *Trials: before reversal*						3 *Trials: after reversal*					
		Noticed			*Did not notice*			*Noticed*			*Did not notice*		
Before reversal	*After reversal*	1	2	3	1	2	3	4	5	6	4	5	6
Up for up	Down for up	2·25	1·65	1·77	2·43	1·65	1·79	7·14	2·83	2·1	1·86	1·82	1·47

These results show that on the whole, subjects who noticed that the directional relationship had been reversed, tended to deteriorate more after the reversal than did those subjects who did not notice. When stimuli were presented at a slow rate all the subjects noticed the reversal. With the discontinuous task the different levels of achievement attained with different directional relationships confuse the picture. For example, for group (*a*) the first part of their task, *before* the reversal was intrinsically easier than the second half, while the reverse was true for group (*b*).

uncertain, or who noticed *something*, but could not specify what change had taken place. These results are summarized in Table III. For each combination of directions chosen the mean error score is given for each group of subjects both before and after the reversal, and the number of subjects falling into each group is also given. These data are taken from Vince (1945) and Mitchell (1946).

As stated earlier, when the task was a continuous one a large proportion of the subjects did not notice the change, and these as a group put up a better performance after the reversal than did the group who did notice; as long as the subject did not notice the change, the relationship between direction of movement of control and display could in fact be reversed without greatly affecting the operator's performance.

In the discontinuous task, subjects always noticed the reversal except when the stimulus rate was as fast as two stimuli per second. (In this experiment stimuli were equally spaced, and the reversal was made between stimuli rather than between trials.) Results from a further experiment at a faster rate, and where the reversal again took place between two trials are given in Table III. Here the records are less easy to assess for the different levels of performance achieved with the different directional relationships confuse the picture. In the main, however, it seems that those who noticed the change (and these were in general the subjects who worked according to a "system," i.e. who had consciously worked out the directional relationship when first tackling the task) tended to put up a better performance before the change, and their performance deteriorated more afterwards, than did the group who did not notice. These differences are however slight, and statistically reliable only in one case. But here, again, it was the subjects who did not notice it who were least affected by the reversal. The conclusion to be drawn from this part of the experiments seems to be that for the discontinuous task a conscious working out of the relationship helped in the early stages of learning (this does not seem to have been necessary in the continuous task), but it could be a drawback in the later stages.

This view of the role of awareness, or the inclusion in awareness of the response, or the direction of the attention towards the response, and a coupling of awareness with a deterioration in performance, is in agreement with the statements of previous writers on the nature of skill (see Bartlett, 1943, and 1947a) and provides a possible clue to differences between these continuous and discontinuous tasks as a whole and a possible explanation of some experimental results outlined above. It seems that when a stimulus requiring a directional response is perceived the subject brings to the task a preformed mental set which enables him to make a response in the same direction as the stimulus, with a minimum of cognitive interference (due to his previous experience). But if the required response is not in the same direction as the stimulus, either the wrong response will be made, or the whole task must be approached with a new mental set. The new mental set will presumably require an awareness of the total situation. But if the wrong response is made, subsequent events may depend on the nature of the task. The correct readjustment may be made almost automatically in a moderately easy task and where the operator has his attention directed towards the stimulus, or the error may, in a difficult task, or with a subject who tends to think of the response, again reproduce the early stages of a difficult learning situation, where sensory and motor aspects of the situation, which have no obvious connection, must be harmonized.

The main conclusion which can be drawn from these results is that as long as the response is efficient, i.e. satisfactorily adapted to the situation, it can be carried on without the attention being directed towards it, but as soon as errors are made, or for some reason the task becomes difficult, the attention is directed towards the response and it may be disrupted further. Cause and effect are not possible to

distinguish, here, however. It is known that awareness accompanies errors but not if awareness precedes (i.e. causes) errors, or results from them, although both may occur. An explanation of the rank order of accuracy obtained with the left hand continuous task could be given in these terms; the use of the non-preferred hand might make the responses unfamiliar, so that the attention would be directed to them rather than to the display alone. Again, in the two-handed continuous task the breaking up of the attention and the conflicting requirements of the two pointers could have had the same effect.

Further evidence which bears on this point is given by Vince (1950). Here, where subjects were given a considerable amount of practice with the "unexpected" discontinuous task and without visual knowledge of results, it was found that learning was accompanied by a decrease in the subject's awareness of his correct responses, and an increase in the speed with which he corrected his errors. That touch typists as they become proficient tend to lose awareness of their responses *except when a mistake is made* has been pointed out by Book (1925).

Awareness of the response, appears, then, to be associated with difficulty or a failure to adapt successfully, perhaps a necessary part of a new skill but one which is useful only in the early stages.

VI

Conclusions

i. In handling any mechanism where the movements of a control knob, lever or handwheel are required to bring about corresponding movements of a controlled part of the machine, there are for the majority of people preferred directional relationships between the control and display. In simple cases "expected" relationships correspond to those encountered in daily life, which are practised by most people throughout their lives. However, there are more ambiguous relationships which correspond less with everyday experience, as when display and control are not in the same plane, and here there is not likely to be unanimity as to which is the "expected" relationship.

ii. These preferred relationships may or may not be reflected in performance. In general, when the subject is not hurried, or is able to concentrate his attention on a task which has but one aspect (as in one-dimensional tracking), the directional relationship is unimportant (or the difficulty inherent in it may be overcome); but a fast stimulus rate or an increase in the complexity of the task, or decrease in its familiarity may cause errors if the directional relationship is "unexpected" or ambiguous (or may render the operator unable to deal with the difficulties inherent in the task).

iii. Under such conditions a task where the directional relationship is "unexpected" will take longer to learn than one where it is "expected."

iv. When performance is affected by a non-preferred relationship, the task becomes one where the cognitive element plays a large part; for this reason there is evidence that such a performance can be predicted on the basis of intelligence; high intelligence correlating with a good performance and a rapid rate of learning and low intelligence with a poor performance and a slow rate of learning.

v. Conditions have been found where it is possible to reverse the relationship between control and display without affecting the subject's performance. This has generally been found to occur only when a rapid series of responses is required, and also only when the subject has not been aware of the reversal.

References

1. Bartlett, F. C. (1943). Fatigue following highly skilled work. *Proc. Roy. Soc.*, B. **131**, 247–257.
2. Bartlett, F. C. (1947a). Some problems of "display" and "control." *Miscellanea Psychologica Albert Michotte.* Louvain.
3. Bartlett, F. C. (1947b). The measurement of human skill. *Brit. Med. J.*, i, 835–838 and 877–880.
4. Book, W. F. (1925). *The psychology of skill.* New York.
5. Craik, K. J. W. (1944). Handwheel designs in C.R.S.I. mock-up. Medical Research Council. R.N.P. 44/164. Unpublished.
6. Fitts, Paul M. (Editor, 1947). Psychological research on equipment design. Washington: U.S. Government Printing Office.
7. Gibbs, C. G. (1950). Factors in the initial learning and transfer of serial motor skills. Medical Research Council. A.P.U. 127/50. Unpublished.
8. Grether, Walter F. (1947). Direction of control in relation to movement in one-dimensional tracking. Army Air Forces Headquarters, Air Materiel Command. Engineering Division TSEAA–8–WGF. Unpublished.
9. Mitchell, M. J. H. (1947). Direction of movement of machine controls. III. Ministry of Supply, S.M. 10018(S). Unpublished.
10. Mitchell, M. J. H. (1948). Direction of movement of machine controls. IV. Medical Research Council. A.P.U. 48/371. Unpublished.
11. Mitchell, M. J. H. (1949). Direction of movement of machine controls. V. Medical Research Council. A.P.U. 110/49. Unpublished.
12. Vince, M. A. (1945). Direction of movement of machine controls. I. Medical Research Council. F.P.R.C. 637. Unpublished.
13. Vince, M. A. (1950). Learning and retention of an "unexpected" control-display relationship. Medical Research Council. A.P.U. 125/50. Unpublished.
14. Vince, M. A., and Mitchell, M. J. H. (1946). Direction of movement of machine controls. II. Ministry of Supply. S.M. 2861 (S). Unpublished.

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THE EFFECT OF KNOWLEDGE OF RESULTS ON LEARNING AND PERFORMANCE

IV. THE DIRECTION OF THE ERROR IN VERY SIMPLE SKILLS

BY

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Re-examination of the data discussed in previous papers of this series shows a greater tendency toward "overshooting" when the time interval between trials, in some of the experiments, is short than when it is longer. The subject tends to make a bigger movement or exert more pressure with short intervals. This seems to be true with or without visual knowledge of results. On the other hand, with the experiments in which the task was to press a key for a given short interval, the effect was not conclusively shown.

A hypothesis is put forward to explain these results in terms of proprioceptive adaptation.

I

Introduction

In the earlier papers of this series (Elwell and Grindley, 1938, Macpherson, Dees and Grindley, 1948, and Macpherson, Dees and Grindley, 1949) we have treated the results in a conventional way, plotting and tabulating errors in learning and performance curves without regard to their algebraic sign. Thus "overshooting" or "undershooting" the target by five units would count as equal errors. A change in the conditions may, however, tend to make the subject give a bigger or smaller response than he did in the previous trial, without necessarily altering his average error, e.g. by making him overshoot the target by the same amount as his previous undershoot. The very wide scatter between individual readings may make such an effect pass without notice. With this in mind we have re-examined the data for the effect of varying the time interval between trials.

Three of the experiments so far reported, namely those that have been called "Knob Turning," "Pressure" and "Time," will be discussed here. The other experiments are omitted because there was no adequate control of the time interval between trials.

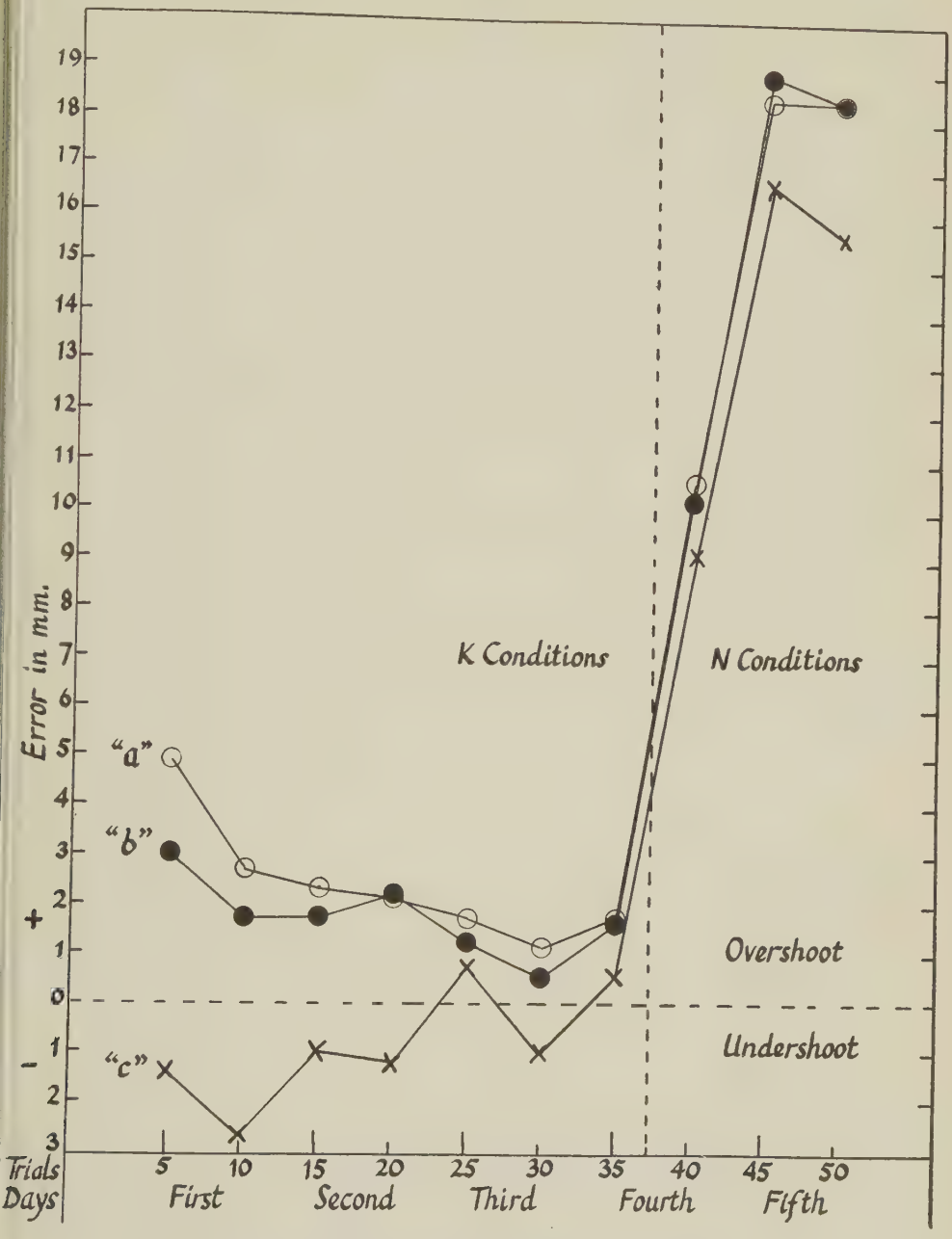
II

Results

A. *Knob Turning.* (For details, see Mapherson, Dees and Grindley, 1949, p. 168.)

In this experiment, in order to hit the bull's eye, the subject's task was to turn a knob through 20° and then immediately return it to zero. He could then, for K ("knowledge of results") conditions be shown the amount and direction of his error by the position of a spot of light on a scale; or for N ("no knowledge of results") conditions, this information could be withheld. A record of his actual movement was made by a pen writer, and it was possible to make the trials as frequent as one every 1·8 seconds. As was shown in the previous paper there was, under K conditions, a very slight tendency to improve more rapidly, in terms of number of trials, with short time intervals than with longer ones. Under N conditions deterioration was more rapid with short time intervals.

FIGURE 1



Each point = Average of 5 trials for 21 Subjects.

⊙ = Trial after 1·8 seconds interval.

● = " " 7·5 " "

× = " " 33·0 " "

If now, taking the same data as before, we include the algebraic sign of the error in our calculations, the general trend of the results shows a very marked qualitative difference between the longer and shorter intervals. Thus for the 21 subjects discussed in the earlier paper, Figure 1 shows the average error for time intervals of 1·8, 7·5 and 33·0 seconds between consecutive trials, the algebraic sign being included. As will be seen from the legend, each point represents the mean of 105 determinations. Half-way through the fourth day the subjects were changed to N conditions. It will be seen by inspection that both for K and N trials, curve "a" (1·8 second intervals) is always above curve "c" (33·0 second intervals), whilst curve "b" (7·5 second intervals) nearly always occupies an intermediate position. This means that after a short interval the subjects tended to turn the knob farther than after a long interval. It will also be seen that with practice under K conditions the curves tend to approximate to a point near the level of the bull's eye.

When knowledge of results is removed the tendency to overshoot is very marked with all three time intervals, but it is even greater in extent with the short time interval than it is with the longer one. The tendency to turn the knob farther the shorter the time interval still persists.

With the wide scatter of individual readings it is, of course, necessary to have some statistical check on the reliability of the results. We have considered than an enumeration of cases in which one "shot" is greater or less than the previous "shot" is an appropriate way of approaching this question. (This avoids any assumption that the scale used is, in any sense, either physiologically or psychologically, a linear one.)

Table I shows the number of cases under K conditions where an increase or decrease of angle of turn occurred, as compared with the immediately preceding angle of turn, for each of the three time intervals. It must be remembered that all the subjects had time intervals in *cycles* in which the order was 1·8, 7·5, 33·0, 1·8, 7·5, 33·0 seconds, and so on.

TABLE I

Time intervals (*seconds*)	1·8	7·5	33·0
Cases of increase in extent	465	334	261
Cases of decrease in extent	239	353	429

Table II shows the corresponding figures under N conditions for the same subjects.

TABLE II

Time intervals (*seconds*)	1·8	7·5	33·0
Cases of increase in extent	272	206	189
Cases of decrease in extent	153	214	241

It will be seen that these tables show a very striking difference between the long and short time intervals. On comparison with the data already published (Macpherson, Dees and Grindley, 1949, pp. 169 and 170) the effect of including the *direction* of the change is quite clear.

Another way of demonstrating the point in question is to compare the data, as shown in Figure 1, with the same data replotted *without* the sign. The latter is done

n Figure 2. Anyone inspecting that figure would be unlikely to think that time ntervals had much effect under K conditions. For N conditions the results are not ery different from those shown in Figure 1 because, in any case, nearly all the trials esulted in "overshoots."

. *Pressure.* (For details, see Macpherson, Dees and Grindley, 1948, p. 75 and 1949, p. 172.)

In this experiment the subject task, in order to hit the bull's eye, was to press a ever against an increasing force until he attained a pressure of about 2 lbs. weight. It was found, as has been discussed in the preceding paper of this series, that here was a very marked tendency to improve more rapidly when the time interval vas short (10 seconds) than when it was longer (60 seconds). When knowledge of esults was removed deterioration was more rapid with the shorter interval.

If we now analyse the results, taking into account the sign of the error, we find just as in the Knob Turning experiment) that there is a very marked tendency to ush harder after the short interval than after the long one. Table III shows the umber of cases of increase or decrease (compared with the previous trial) of pressure xerted with each interval under K conditions.

TABLE III

Time intervals (seconds)	10	60
Cases of increase of pressure	135	40
Cases of decrease of pressure..	40	118

Similarly Table IV shows the number of cases under N conditions.

TABLE IV

Time intervals (seconds)	10	60
Cases of increase of pressure	447	282
Cases of decrease of pressure..	293	435

Over 75 per cent. of the trials, under N conditions, were "overshoots" and in many ases the overshooting was so great as to be beyond the range of the scale used. When two successive shots were both off the scale this pair could not be included n Table IV.)

. *Time.* (For details, see Macpherson, Dees and Grindley, 1948, p. 72, and 1949, p. 171.)

In this experiment the subject's task was to press a morse key so that, in order o hit the bull's eye, he had to make contact for about 0·7 second. When he had eleased the key his accuracy could be shown to him on a galvanometer scale. As as been pointed out (Macpherson, Dees and Grindley, 1949, p. 171), intervals of 12 nd 60 seconds between trials produced only a slight difference in their effect under oth K and N conditions. Re-examination of the data, taking the sign of the rrors into account, also fails to show any convincing difference between the effects f these two time intervals. This may possibly be due to the fact that the movement

only took about 0·7 second, and the shortest interval between trials, owing to apparatus difficulties, was 12 seconds. It is possible that had there been greater massing of trials different results would have appeared, but on this we have no evidence.

D. *The Effect of 24-Hour Intervals.*

In all three of the experiments discussed above, the subjects were given series of trials on each of four or five successive days. We therefore have some data on the effects of a 24-hour interval under both K and N conditions, although, obviously the number of cases which can be taken into account is much smaller than those for the shorter intervals already considered. For instance, if one subject has 31 trials on each of five days (as was the case in the Knob Turning experiment), we can make a total of 150 comparisons per subject for the time intervals discussed above; but we can only make 4 comparisons per subject for the effect of 24-hour intervals.

Inspection of the data, while confirming the tendency described in previous papers of this series, to become less accurate during a 24-hour period, does not show any consistent trend in the (algebraic) direction of this deterioration. This may be merely owing to lack of sufficient data, but if there was any very strong directional tendency some indication of this might have been expected to show in the results.

III

Discussion and Conclusions

The results of previous experiments, which are reviewed in this paper, show very clearly the importance of taking into account the direction as well as the amount of the error in any curve of learning or performance. For instance, in the "Knob Turning" experiment, if we consider only the amount of the error, as in Figure 2, we should infer that the time interval between trials has very little influence on the results. If, however, we include the algebraic sign of the error, as is done in Figure 1, we see at once that the time interval has a very large effect. This, apart from its application to the present problems, is a general point in the methodology of the study of learning and performance. Taking the analogy of psycho-physical experiments, this is only like saying that the Constant Error as well as the Average Error should be considered.

More interesting, perhaps, are the hypotheses suggested by the results when the direction of the error is taken into account. *One* such hypothesis may be roughly summarized as follows. Let us assume:

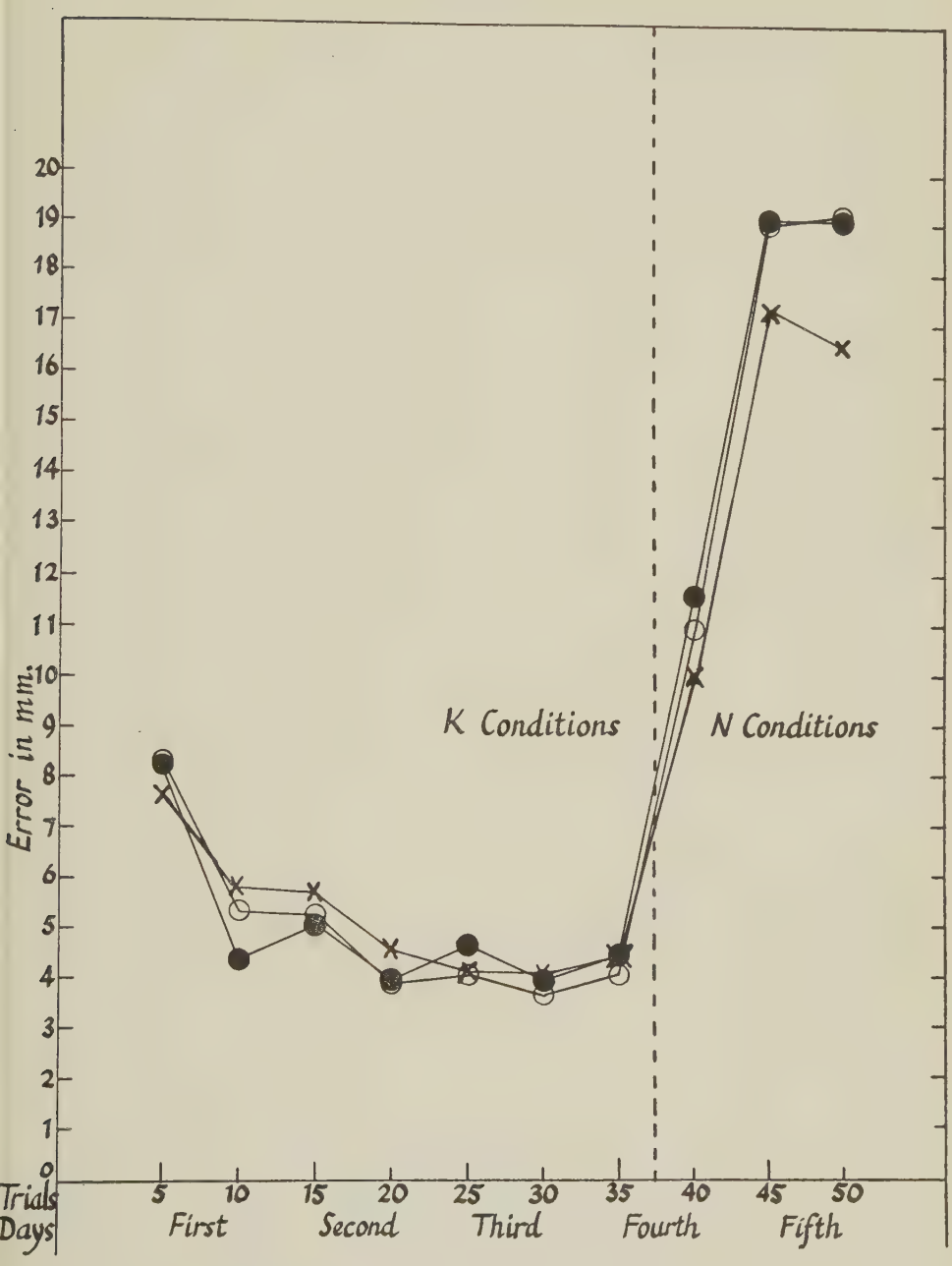
(*a*) That when a subject is trying to repeat a movement (e.g. when he has hit the bull's eye in the previous trial) he is trying to obtain proprioceptive sensations which "match" (cf. Bartlett, 1932, p. 20 et seq.) his "memory trace" of those in the previous trial.

(*b*) That the proprioceptive mechanism is susceptible to adaptation (cf. Adrian, 1928), i.e. during the period immediately following a response the receptors, or some more central part of the mechanism will be less sensitive. The degree of adaptation will, of course, decline with the time since the last response.

(c) That the "memory trace" left by the proprioceptive stimuli (like those of some visual forms studied, e.g. by Tsao, 1949) becomes less precise with lapse of time, but that there is no marked directional trend in such forgetting.

Making these assumptions, none of which seems to be improbable, it at once becomes clear why in the Knob Turning and Pressure experiments a short interval

FIGURE 2



Each point = Average of 5 trials for 21 Subjects.

⊙ = Trial after 1·8 seconds interval.

● = " " 7·5 " "

× = " " 33·0 " "

between trials tended to produce a greater response in the second trial than in the first one; since, owing to the adaptation of the proprioceptive system, the second response, when it reaches equality with the first, would still feel too small to the subject and he would tend, therefore, to increase it. (Any corrective action towards the direction of undershooting would not have been registered on any of the pieces of apparatus discussed—since each of them recorded only the *maximum* reached.) With longer time intervals (e.g. 30 seconds) the adaptation would have diminished. And with very long intervals (e.g. 24 hours) would have disappeared.

With the Time experiment this adaptation effect would not be expected to any marked degree because of the very short duration of the subject's movement compared with the minimum interval which was possible between trials. It may also be that the short absolute duration of the movement made correction while the action was in progress impossible or very difficult (cf. Hick, 1949, and Vince, 1948).

The hypothesis suggested makes no claim to explain the general mechanism of "learning," or the differences between what have been called K and N trials, or the "incentive" and "directive" effects. A complete theory should obviously link these findings with the many investigations of phenomena such as the "time error," muscular "after contraction," and "positive" and "negative" after images. But since these investigations do not yet form a coherent whole, we have not attempted to discuss them here; and have confined ourselves to a simple hypothesis about our present data. What this suggests is a possible explanation of the observed effects of time intervals between trials; it is, of course, incomplete and unproved.

Several other lines of approach to the problem would be possible, but the justification for this one is that it seems to fit with a number of observed facts. Also, since the suggested locus for the underlying process is in the proprioceptive mechanism, it makes possible some predictions about what ought to happen in more complex skills, where more than one muscle group, or more than one limb, is involved.

We wish to express our thanks to Professor Sir Frederic Bartlett for many suggestions, and to the Rockefeller Foundation for financial aid.

References

1. Adrian, E. D. (1928). *The Basis of Sensation.* London.
2. Bartlett, F. C. (1932). *Remembering.* Cambridge. P. 20 *et seq.*
3. Elwell, J. L., and Grindley, G. C. (1938). The effect of knowledge of results on learning and performance. I. A co-ordinated movement of the two hands. *Brit. J. Psychol.*, **24**, 39–53.
4. Hick, W. E. (1949). Reaction time for the amendment of a response. *Quart. J. Exp. Psychol.*, **1**, 175–179.
5. Macpherson, S. J., Dees, V., and Grindley, G. C. (1948). The effect of knowledge of results on learning and performance. II. Some characteristics of very simple skills. *Quart. J. Exp. Psychol.*, **1**, 68–78.
6. Macpherson, S. J., Dees, V., and Grindley, G. C. (1949). The effect of knowledge of results on learning and performance. III. The influence of the time interval between trials. *Quart. J. Exp. Psychol.*, **1**, 167–174.
7. Tsao, J. C. (1949). A study in the recognition of figures. *Brit. J. Psychol.*, **40**, 57–67.
8. Vince, M. A. (1948). Corrective movements in a pursuit task. *Quart. J. Exp. Psychol.*, **1**, 85–103.

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A PRELIMINARY REPORT ON A NEW AUDITORY AFTER-EFFECT

BY

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It is found that protracted monotic stimulation at a certain frequency lowers the DL of that frequency for the contralateral ear by some 47 per cent. There were eight subjects and six controls; the frequency was 1,000 c.p.s. at 40 d.b.s. It is suggested that this after-effect is analogous to figural after-effects in vision. (Some further related findings, as yet preliminary, are also reported).

I

Introduction

Köhler and Wallach show that changes occur in the visual field after prolonged uniform stimulation and that these are central (1944). If a figure is fixated for a prolonged period, other figures falling on or near the same part of the visual field immediately afterwards recede from where the contours of the first figure were first located. It is therefore of theoretical interest to see whether analogous changes occur in other sense modalities and more especially in hearing.

II

Method

(1) *General considerations:* The actual methods of determining this was to measure the DL monotically at a certain frequency (1,000 c.p.s.) and then to stimulate the other ear at the same frequency for a prolonged period. Though not providing a direct test the method is simple and uses a minimum of apparatus. It seeks to verify the prediction based on the visual analogy in the following way.

A contour in visual space tends to push away other contours and make the interval between them seem larger. Therefore a tone in the auditory scale ought to push away the neighbouring tones and increase the tonal interval between them. Now it has been found that the ratio of the physical magnitude of equal-appearing intervals is roughly equal to the ratio of the physical magnitude of the DLs found respectively in the two intervals (Stevens and Davis, 1938). If then the phenomenal tonal distance in any region of the frequency scale does increase, the size of the DLs there ought to decrease. The DL at a certain frequency should diminish after prolonged stimulation at that frequency.

(2) *The procedure of investigation:* The procedure had two aims:

(*a*) to secure a clearly central effect;

(*b*) to avoid too much stimulation when testing for the DL initially, for fear of introducing the predicted after-effect prematurely.

In order to achieve the first aim, a tone of 1,000 c.p.s. was sounded in one ear through one of a pair of earphones, 40 d.b.s. above threshold. This intensity was used in both the DL determinations and also in the prolonged contralateral stimulation. A tone of this intensity is inaudible through the other ear under these conditions. The tone was sounded by pressing a switch.

In order to achieve the second, the method of minimal changes was used in preference to the method of constant differences, as it is more economical. Eight series, four ascending and four descending, were generally presented to each subject in order to determine the initial DL. The two notes were always played in the same order, i.e. the higher note was always sounded after the note of 1,000 c.p.s. The subject was instructed to report either "same" or "different" in pitch. These further points in procedure were adopted because they proved to be the least confusing to naïve subjects.

The subject was seated facing away from the experimenter and the control dial. Each tone was presented for approximately one second. The tone to be compared was presented about half a second later for the same length of time. These times are approximate because the switching was done by hand.

After the DL had been thus determined, the tone of 1,000 c.p.s. was sounded in the contralateral ear (monotically) also at 40 d.b.s. above threshold for four minutes.

At the end of this period, the DL at this frequency was again determined for the original ear, using exactly the same procedure as in the first instance. With some subjects more than eight series were presented.

The control group was subjected to the same procedure, except that a note of 600 c.p.s. 40 d.b.s. above threshold was sounded instead of the one at 1,000 c.p.s. The experiment was not carried out in a sound-proof room.

(3) *Subjects:* The experimental group consisted of eight subjects roughly between the ages of 18 and 24. The control group consisted of six subjects of the same age group. Both groups were naïve, i.e. they did not know what results to expect. Some subjects who were hopelessly inconsistent in making judgments were rejected both as subjects and controls. A subject was deemed to be inconsistent when the distribution of "same" judgments was non-modal.

III

Results

After the DLs both before and after contralateral monotic stimulation had been obtained, it was determined by Student's *t* test, using a formula for small numbers, whether the DL for each subject was significantly lower in the second instance than in the first. The 0·05 level of confidence was adopted as significant.

TABLE I

Results of Experimental Group

Subject	*DL before stimulation (in c.p.s.)*	*DL after stimulation at* 1,000	*Decrease in c.p.s.*	*% Decrease*	*t, with degrees of freedom*	*Whether significant*
1	1009·5	1005·65	3·85	44·74	4·9 22 df	yes
2	1007	1003·88	3·12	44·57	2·63 22	yes
3	1006·75	1003·91	2·84	42·33	6·89 17	yes
4	1019·7	1010	9·7	49·24	4·93 16	yes
5	1012·5	1006·5	6	48·00	4·03 14	yes
6	1013·25	1005·8	7·45	56·33	2·16 14	yes
7	1011	1006·7	4·3	39·10	2·40 13	yes
8	1002·37	1001·32	1·05	44·43	2·08 18	no
Average	1010·26	1005·44	4·82	46·98		

The results for the experimental group are set out in Table I. Of the eight subjects in this group, seven showed a significant decrease. The one subject who did not show a significant decrease, nevertheless showed a decrease.

The results of the control group are given in Table II. The six subjects in this group varied randomly, some in the direction of a diminution of threshold, others fluctuating upward.

TABLE II

Results of Control Group

Subject	*DL before stimulation (in c.p.s.)*	*DL after stimulation at 600*	*Decrease in c.p.s.*	*% Decrease*	*t, with degrees of freedom*	*Whether significant*
1	1008·75	1007·5	1·25	14·28	0·88 14	no
2	1005·75	1007·25	−1·5	−26·08	1·44 14	no
3	1009·25	1008·25	1	10·08	0·78 14	no
4	1013·5	1014	−0·5	−3·70	0·35 14	no
5	1008	1007·5	0·5	6·25	0·41 14	no
6	1002·25	1002	0·25	11·11	0·14 14	no
Average	1007·92	1007·75	0·17	2·14		

It is clear from the results that there is marked decrease in DL in the experimental group.

The control group had an initially lower DL than the experimental group. But it is found that this variation between the groups could quite easily occur by chance ($t = 0{\cdot}935$ with 12 degrees of freedom, even when we treat each mean as a measurement and ignore the scatter). Therefore the control is a true one.

IV

Further investigations of the effect (which are as yet preliminary):

These indicate that:

(*a*) The DL can be lowered in this manner at any heard frequency.

(*b*) Both the ascending and the descending DLs decrease by the same amount.

(*c*) A whole small range surrounding the prolonged stimulus tone becomes more sensitive to alteration of pitch.

(*d*) Subjects whose DL for a particular frequency has been lowered by the described procedure, exhibit a rapid return to the lower DL in the course of re-testing 24 hours later. This occurs without any contralateral stimulation and merely as a result of the stimulation inherent in the DL determination itself.

(*e*) If a subject is asked to bisect a tonal interval before and after contralateral monotic stimulation on the base note of the interval, the estimated midpoint shifts towards the base note.

V

Discussion

The contention may be held that the results obtained are due to the practice effect, an already well-known factor in DL measurement. This is clearly invalid. Both the experimental and control groups had the same amount of practice in the initial determination. If one may speculate on the basis of these results, may not the so-called practice effect be in reality due to prolonged stimulation and not to any practice or learning?

Wyatt (1945) claims that pitch-discrimination can be improved with training at particular frequencies, with some transfer to the octaves. The present findings suggest an alternative causal factor, and one which, if taken into consideration, would explain the divergence of experimental results on the influence of training in discrimination. If sharp improvement occurs as a result of prolonged stimulation "training" may or may not have an effect depending exactly on the amount, duration, and spacing of the "practice." This might give rise to "contradictory" results.

The after-effects discovered in this experiment would indicate that there is something analogous to visual figural after-effects in hearing.

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References

1. Köhler, W. and Wallach, (1944). Figural after-effects. *Proc. Amer. Phil. Soc.*, **88**, 4.
2. Stevens, S. S. and Davis, H. (1938). *Hearing*. New York. pp. 95-8.
3. Wyatt, R. F. (1945). Improvability of Pitch Discrimination. *Psychol. Monogr.* **58**, No. 2.

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BOOK REVIEWS

Physiological Mechanisms in Animal Behaviour. [Symposia of the Society for Experimental Biology, No. IV.] Cambridge University Press, 1950. Pp. vii + 482. 35s. net.

It is an old saying that there are no *real* frontiers between allied sciences. If we make use (as we all do in our teaching or research about the living organism) of a large and—perhaps deplorably—increasing number of titles for different branches of study, this can only be justified on grounds of practical or administrative convenience. The present book illustrates the point well in the case of the relationship of "psychology" to "physiology" or "zoology"; for many of the authors would claim to speak only as specialists in particular branches of one or the other of these sciences. Yet the result, if not a completely coherent whole, serves to emphasize the similarities rather than the differences between the methods and results of people who work under quite different academic labels.

The book gives the text of the papers read at an international symposium (organized jointly by the Society for Experimental Biology and the Association for the Study of Animal Behaviour), which was held in 1949. As there were 21 contributors, discussing different things, coming from different countries, and each giving his own views on his own subject, space does not permit anything like the detailed commentary which most of these articles deserve. The book, however, is divided into four sections, about each of which a word may be said.

The first, on "The Range and Capabilities of the Sense Organs," contains very valuable summaries—by authoritative writers—of present views on hearing, vision, proprioception, and the functions of the labyrinth. While admiring these one may regret that time did not permit similar contributions on the tactile, thermal or chemical senses.

In the second section, on "Central and Peripheral Control of Behaviour Patterns," we have the exciting mixture of papers by E. D. Adrian, Paul Weiss, James Gray, G. P. Wells and E. von Holst. There is nothing which could be called an agreed conclusion, but there is much in each which needs a lot of thought, e.g. the usually super-cautious Adrian feels driven to say ". . . in the cerebral cortex there are no through routes. . ."

The third section, on "Instincts, Taxes, etc.," is a rather mixed bag. It contains eight papers which vary from the very specific (e.g. P. H. T. Hartley on recognition in birds—a paper which has immense implications for the theory of perception) to the very general views of K. Z. Lorentz on "Comparative Method" or of N. Tinbergen on "Hierarchical Organization." C. F. A. Pantin's account of the lower invertebrates, or E. A. Armstrong's essay on "Displacement Activities" (in birds) would each need separate reviews.

The last section, on "Learning," contains only four papers, but all of these are likely to be interesting to most psychologists. W. H. Thorpe surveys the whole problem from what might be called a "Lloyd Mogan + Thorndike + Lorentz" point of view, while J. Konorski does much the same from a Pavlovian angle. Then come R. B. Boycott and J. Z. Young with the octopus, and the intriguing effects of lesions in its nervous system. Finally we have K. S. Lashley "In Search of the Engram," and we must admit that in its power to demolish current theories the old scalpel has not lost its sharpness. One remark will suffice as an illustration: "I sometimes feel, in reviewing the evidence on the localization of the memory trace, that the necessary conclusion is that learning just is not possible. . . ." Nevertheless, Lashley goes on to say ". . . learning does sometimes occur." Perhaps one is wrong in being reminded of Galileo or Einstein.

Anyone who wants to know what biological experts think about behaviour and the causes of its many varieties ought to have this book. But it is not a text-book; it is a collection of views. Those who attended the symposium probably have happy memories of the lack of distinction between academic "subjects," as well as of the high standard of discussion. Thanks are due to J. F. Danielli and R. Brown for their admirable work as editors.

It is worth adding that during the period of this symposium a series of "round-table conferences" was held to discuss nomenclature in animal behaviour, in an attempt to find whether a more uniform terminology could be agreed. The terms discussed included many which are commonly used in general psychology (e.g. "instinct," "inhibition," "conditioning," "learning" and "insight"). A concise summary of these discussions has been prepared by W. H. Thorpe, and will shortly appear in the *Bulletin of Animal Behaviour.*

G. C. G.

Handbook of Psychological Research on the Rat. By N. L. Munn. London: George G. Harrap & Co., Ltd., 1950. Pp. xxvi + 598. 35s. net.

This is a most valuable and comprehensive summary of work on all aspects of rat behavior. It is well arranged, and has an admirable bibliography of about 2,500 papers. The title is much more appropriate than that of the original "An Introduction to Animal Psychology" (1933)—of which this is a much expanded and much more up-to-date version. The book is about rats only; but about them it is encyclopædic.

Proceedings of the Experimental Psychology Group 1949-50

10th December, 1949.—3rd Annual General Meeting in London. "The Comparative study of Psychological and Physiological Effects of Electroshock Convulsions," by R. W. Russell. "'Equivalent Rooms' with some Remarks on the Transformations involved in Visual Perception," by R. C. Oldfield.

20th May, 1950 (London).—"The Subjective Judgment of the properties of Deformable Materials," by R. Harper. "The Testing of Abstraction, with particular reference to Deterioration in Schizophrenia," by K. R. L. Hall. *2nd Session:* "The Experimental Study of Rumour," by T. M. Higham (by invitation).

21st-22nd July, 1950.—Extended Meeting at Cambridge. *1st Session:* "Experimental Finger Dyspraxia," by C. V. Jackson (by invitation) and O. L. Zangwill. *2nd Session:* "Programme for Experiments on Thinking," by Sir Frederic Bartlett (by invitation). *3rd Session:* "Perceptual Anticipation in Harmonic Tracking," by C. Poulton. "A Probability Approach to Trial and Error Learning," by A. R. Jonckheere (by invitation). *4th Session:* "Investigation into changes in Autonomic Responses following Pre-Frontal Leucotomy," by A. Elithorne and M. F. Piercy (both by invitation).

29th-30th September, 1950.—Extended Meeting at Oxford. *1st Session:* "Derived Activities," by N. Tinbergen (by invitation). *2nd Session:* "Speed Stress," by R. Conrad (by invitation). "Variation in Reflex Blink Rate during Visual-Motor Activities," by G. C. Drew. *3rd Session:* Short reports on work in progress at the Oxford University Institute of Experimental Psychology by J. A. Deutsch, R. F. Benton, K. Danziger and A. J. Weir (all by invitation).

16th December, 1950.—4th Annual General Meeting in London. "The EEG in the Study of Personality Disorders," by D. A. Pond (by invitation). "Some material for a Discussion of the Constancy of Size," by R. B. Joynson.

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